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On the Testing of Chain Cables. By FREDERICK ARTHUR PAGET, Esq., C.E.

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(Continued from page 152.)

THE question as to the re-testing of cables that have been in use for a certain time is yet unsettled, but the inquiry is of scarcely less importance than that of the first testing. There are many applications of wrought iron in which it is subjected to impulsive stresses, often more or less accompanied by vibrations, and in which, nevertheless, the detail or structure has to conform to certain narrow limits of size and weight. Such is the case with most applications of chains; for instance, to cranes, inclines, forge-slings, &c. Such is the case also, more or less, with railway axles; the axles of carriages on rough common roads; the gags of helve hammers; the porter bars fixed to the blooms whilst under the hammer; the iron wires of some piano-fortes; and many similar applications of wrought iron. The simple fact that only one-half of the gradually applied stress required to produce the proof strain will, if applied suddenly, of itself produce the proof strain (which if exceeded would injure the piece), goes a long way in explaining the matter. Where great interests of life and property are involved in the safe action of these applications of iron, the irresistible logic of facts has sometimes caused preparatory allowances to be made for these "fatigues of the metal." The axles of the London omnibuses are

stated to be always renewed after having run a certain fixed mileage. This system is also carried out with the carriages of the Messageries Générales, the axles of which are changed after having run a limit of 40,000 kilométres. The Honorable the Corporation of the Trinity House entirely renews all the moorings of the light-ships every four years—one-fourth of the number yearly. This limit of time gives the measure of the perfect efficiency of a good cable, well proportioned to its work, and in constant use day and night. Cables in ordinary ships are of course much less, or rather much more slowly, subject to deterioration. We have seen that M. David fixed the time after which a cable in ordinary use should be tested at ten or twelve years. Mr. Macdonald, of the Liverpool testing house, stated, before the 1860 Committee, that he would examine a cable after any long voyage—such as to India or Australia. The late Mr. Green, the great ship-owner, explained that this was done with the mooring tackle of all his ships. An experienced pilot, Mr. G. J. Thompson, said that it should be made imperative to re-test chain cables every six years, and Mr. Smale fixed this limit at seven years. Mr. J. R. Clarke, however, the chief clerk of the store office, stated that there were many sound cables in store twenty years old. It is clear that it would be very difficult to fix a limit of time that could be applied to all classes of ships. The cables in the royal ships are scarcely so often or so severely tried by use as those of some merchant vessels. A cable might remain good for many years, and yet at last be injured in a single storm. Apart from accidents, such as abrasion on rocks, or against a sharp-cornered anchor stock, or similar causes, there are three main conditions affecting the duration of cables and furthering their progressive deterioration under wear:—1st, the friction and abrasion at the crowns; 2d, rust and corrosion by the sea-water; 3d, undue strains on the cable, and in excess of the compressive and tensile elastic limits of the materials. The average amount of abrasion and consequent wear at the crowns could only be determined by a statistical comparison of the deterioration of a number of cables, worked under similar circumstances, through a certain period of time. No full observation of this kind seems to have been yet made. The same appears to be the case with the deterioration of iron cables by rust and corrosion. Mr. Mallet has observed, "that the metallic destruction by corrosion of iron in sea-water is a maximum in clear sea-water of the temperature of 115° Fahr., that it is nearly as great in foul sea-water, and is a minimum in clear fresh river-water." It also appears that, the finer and more equable the quality of the iron, the slower is its corrosion. The alternative action of the air and the sea-water in ordinary cables must have some influence on their deterioration. Again, at a depth of, say, 100 fathoms, there would be a pressure of nearly 17 tons on the square foot, and this pressure would search out any slight crevice, or any slightly defective weld that had escaped the test. It is at these places that the corrosive action of the water is most felt. It is a well ascertained fact that the spongy mass of mechanically compressed crystals we call wrought iron, is porous, as water can be forced through it at compara-

tively moderate pressures. It is also well known that the hydrated oxide of iron we term rust performs the part of an electro-negative element when in contact with metallic iron, which is then electro-positive. When iron is rusting in the air, the moisture of the atmosphere is the exciting liquid, but this voltaic action must be greatly intensified in the presence of sea-water. I have noticed the interesting fact—which deserves more investigation than I have yet been able to give to it—that in the links of a great number of chains the wrought iron is much more eaten away at the sides, where it is in contact with the cast iron cross-stay. The same action was stated, in a number of the *Times* of last year, to have been observed on wrought iron tie-rods in contact with the plates of a cast iron sea-water tank which burst last June at Woolwich. I had lately occasion to examine a number of old chains, after they had been cleaned, and after the rust had been knocked off with a hammer. All the cast iron cross-stays, almost without an exception, were slack. Each link was thus temporarily reduced to the condition of an unstayed link, the ultimate strength of which, compared with a stayed link, is generally taken to be in the ratio of 7 to 9. When the cable is in use, the progress of this undoubtedly voltaic action in weakening them will be aided by mechanical causes. The rust generated between the cross-stay and the sides of the link will be more or less washed out by the surge of the cable; a sufficient longitudinal stress would cause the virtually unstayed link to collapse on the stay; the sea-water would again search out the chinks; would again decompose the material; and the deterioration of the cable thus chemically and mechanically weakened, would progressively advance in successive increments that would render its ultimate destruction a mere matter of time. This action would be, of course, more felt in a cable in constant use, such as those of the Honorable the Corporation of the Trinity House; and whether zincing, which is stated by Dr. Parry to prevent rust, would be of any use, or whether other means, which will doubtless occur to many here, might prevent, or at least modify, this action, is perhaps, a question worthy of investigation by the able men comprising the Trinity Board. There is, however, no need to search amongst the mysterious forces of nature for the main cause that leads to the ultimate destruction of a cable, or of any other application of iron, under like conditions. The primary cause of the destruction of a cable is simply due to the limit of elasticity of its material being exceeded. All chains are, by their very structure and special uses, subject to jerks and shocks; any country blacksmith knows that a chain that can stand a dead pull, would give way under the same weight if suddenly applied; and we all know that a careless laborer at the winch handle of a crane sometimes breaks down a good chain by a heedless jerk. Little more than $5\frac{1}{2}$ tons to the square in. if suddenly applied, would at once bring on the proof strain of 11·46 tons; and although the dead weight of a cable is its greatest safeguard —so much so, in fact, that if the cable out of the hawser could be weighed at different parts of its length, this would be an advantage—yet, nevertheless, the safe load of about $2\frac{3}{4}$ tons, under an impulsive

stress, to the square inch, must be often exceeded in practice. The safe load under an impulsive stress is in truth rather less, as the assumption is based upon the usual notion, which assimilates a cross-stayed link to a couple of bars.

It appears a paradox to say that the chain is, in one sense, strengthened by a strain in excess of the elastic limit, but such is the fact. The power to bear a static load is indeed increased, as was shown by the experiments cited, before the Committee of 1860, to prove that a cable is strengthened by being broken several times under the gradually applied load of the hydraulic press; and, as was also shown by the performances of the $1\frac{3}{8}$th bars subjected to the same treatment by Mr. Loyd. The link is, in the first place, mechanically strengthened by being drawn into a lozenge-like shape, as the two sides of each end then act as ties to a very short beam; but this is obtained at the expense of the elasticity of the material—the material of the link is rendered harder. It is a somewhat fanciful analogy to compare the limits of elasticity and of rupture of iron to the organic life of a plant or animal, but it is justified by the common expression that a bar is said to be crippled by an undue strain. If this living force in a bar—these *forces vives de resistance*, as they are termed by Poncelet—if, in one word, the work to be done in stretching a bar be expressed (in the English way shown by Mr. Mallet) by multiplying half the static load in pounds required to stretch a bar one foot long and of one inch cross-section to its limit of elasticity, by its elongation in terms of a foot (T_e); and if the static load required to break the bar be expressed in the same way—by multiplying half the static load in pounds by the ultimate elongation in terms of a foot (T_r)—we shall then get the power for work expressed in foot-pounds, or the structural value of our bar, and shall see the reason that a chain may be crippled for any application in which it is subject to an impulsive force. The short range, multiplied into the high static load required to stretch a bar of hard iron to its limit of elasticity, compared with the product of the long range but low static load required to stretch a bar of soft, ductile iron, will show that a link made of hard, brittle iron will keep its shape much better than one made of soft, ductile iron. A calculation of the work done in rupturing a bar of soft iron will show that its living force of resistance to rupture is several hundred times greater than the force required to alter its elasticity; and a similar calculation of the work done in rupturing a bar of hard iron will show that the work to be done in breaking it is perhaps twenty times less than that in stretching it to its elastic limit. As any impulsive force is equal to twice the work to be done in producing or consuming it, and as the effort required is less as the distance gone over is greater, it will be seen that, although resilience is a *sine quâ non* in a cable, the strength of the links would be destroyed, and the structural flexibility of the whole cable would be injured, by the use of iron too soft; while the use of very hard iron in the first instance, or the ultimate hardening of any iron when its limits of elasticity are exceeded, renders a cable of hard or hardened iron utterly useless for its intended purpose.

There is thus no necessity to have recourse to any theory of the crystallization of iron under impulsive stresses to explain the gradual deterioration of a cable; but this question of crystallization is one of the greatest importance and interest; and we may yet learn that the structural value, for many purposes, of a given bar of iron is in some determinate relation to the size of the facets of the crystals of which it is composed. A good cable bar consists of crystals that have been more or less elongated while passing through the rolls; the question is whether these crystals are loosened or separated at their planes of cleavage, or whether the crystalline axes have been transposed, under the undue strains, more or less accompanied by vibration, to which chains in general, and chain cables in particular, are necessarily subject. There is no well-ascertained instance of any alteration of this kind happening under moderate stresses, but Mr. Mallet appears to believe that a reversal of the crystalline axes takes place when the elastic limit either of extension or compression, and therefore of flexure, is exceeded, and more especially if the piece be not initially in a state of molecular repose. There is every reason to believe in the existence of internal strains in the link of a chain, and more especially at the crowns. But numberless experiments by Dr. Rankine and others, and more particularly by Mr. Kirkaldy, have shown that what is popularly called a crystalline fracture may be given to the most fibrous piece of iron if it be broken under a suddenly-applied load—an effect simply due to the mechanical effect of a sudden stress, and to the fact that any piece of iron is an assemblage of crystals. There is no reason to believe that a magnifying glass—as was, indeed, shown by Robert Stephenson—would reveal any material difference between a bar broken after fatigue of whatever kind, or a bar broken when fresh from the mill. At the same time, the application of a very powerful miscroscope to the molecular structure of iron has yet to be made; and the history of the first application of the telescope to a very different science may yet find its counterpart in this department of physical knowledge.

Whatever be the internal effect of the lateral contraction induced by excessive tensile strains, it would be of the utmost importance to settle, once and for all, whether re-annealing can restore the living force of resistance of iron, and, therefore, of a cable. Mr. T. M. Gladstone, C.E., recommended this plan before the Committee of 1860. Mr. Smale, then of Woolwich, said that this would be like Burnetizing rotton wood. Dr. Noad, in a letter to the *Times*, about eight years ago, stated that he had taken away the brittleness of an old chain by keeping it for 24 hours in a furnace. The late Mr. Glynn recommended that a crane chain should be annealed every three years. At the North Roskear mine, in Cornwall, it is stated by M. Moissonet, that the pit-chains are withdrawn from the shaft after every six months use, are rolled in a heap, then covered with a sort of cylindrical furnace, and brought to a red heat. According to an account translated from the Russian into the *Polytechnisches Centralblatt*, the chain cables for the Russian government, after being brought to a dark-red heat

immediately after testing, are then tarred—a plan which is said to prevent rusting, as the tar thus takes a firmer hold on the iron. But many things may be done with charcoal iron that it would not be safe to attempt with our ordinary iron. Baudrimont appears to believe that all metals only acquire determinate qualities by proper annealing, and that a cherry-red heat is necessary for annealing wrought iron. According to the experiments by the Franklin Institute, wrought iron is perfectly annealed at a clear bright red. The experiments of both Baudrimont and the Franklin Institute show that the ultimate tenacity of iron is considerably diminished by annealing, but, unfortunately, in neither case was the elongation noticed. Poncelet has shown that his co-efficient, T_e, of elasticity is increased with annealed iron, but that the co-efficient of rupture, T_r, is diminished. This refers to wires, and no complete experiments appear to have been yet made on the effect of annealing on bars. It is a question whether the extra ductility conferred on the links by the process of annealing would not, while rendering them more ductile, at the same time lead to their changing their form. At any rate, at least some of the cast iron cross-stays would be rendered less able to withstand distortion. At the same time, the question ought to be settled, and to cables comparatively uninjured by corrosion, the process might prove of great value. The conditions of size in a cable are peculiarly favorable to the use of annealing. Great as the advantage would be in the successful application of annealing to large forgings, there are several well-authenticated instances of massive crystals being developed in the interior of the mass by the long-continued action of a red heat. General Morin thus mentions an instance of the production of crystals, with facets from 4 to 5 millimetres in breadth, in a charcoal iron bar originally of fine, soft, fibrous, texture.

Tied as we are in testing cables within a narrow limit, which if exceeded in either direction would, on the one hand, either impair the efficiency of the cable, or on the other, the efficiency of the test, it is clear that the most thorough accuracy is required in measuring the proof stress. Unfortunately, it is not always the case that this accuracy is obtained. The stress exerted by the machine of M. David, of Havre, was shown by the French government to be taken too high. The appliance for the measurement of the stress exerted by the Liverpool corporation testing machine, was a few years ago shown by Mr. Mallet to give a result of nearly 9½ per cent. error in excess. Some of these machines consist of a powerful windlass purchase, but we will confine our attention to the direct-acting hydraulic press, the application of which to the testing of chain cables, by the late Sir Samuel Brown, may be said to have rendered the iron cable a practicable thing. There are three distinct ways of measuring, or at least approximately measuring, the stress exerted by the press plunger. 1st. A small valve is fitted to the cylinder and furnished with a steel-yard and adjustable weight. In large machines this is, for the sake of convenience, carried to a distance from the press, the water being conveyed in a small pipe. 2d. A Bourdon gauge is attached in the same way, either

direct on the cylinder, or it is placed in communication therewith by means of a small pipe. 3d. The other end of the chain being tested is attached to the head of a bent iron lever, the power of which is multiplied by a system of levers balanced on knife edges. The plan of measuring the stress exerted by the press plunger, by means of a weighted valve, is liable to several objections, as was pointed out many years ago by Professor Peter Barlow, more recently by Professor Rankine, and by Mr. Bowman in his evidence before the 1860 Committee. In the first place, the relative proportion between the pump plunger and the valve is necessarily great; and a simple calculation will show that a hair's breadth more or less to the valve would make an important difference. In the next place, the friction of the leathers and the weight of the plunger are not taken into account; the gross load on the plunger is, in fact, given as the useful work at the end of the piston rod. Some experiments made by Professor Rankine, whose name is a sufficient guarantee in matters of this kind, have shown that about one-tenth should be deducted from the pressure in the hydraulic press, merely for the friction of the press plunger. The real, the useful work exerted at the end of the plunger on the chain is thus more than 10 per cent. less than is given by the pressure of the water. An error in the opposite direction will be made by conducting the pressure of the water, either on a weighted valve, or on a Bourdon gauge, and this error will vary with the diameter of the pipe, the number of bends, and the other losses of effect in a stream of water passing through a pipe, which are well known to engineers. The load on a safety valve is always an unreliable datum for computing pressure; a Bourdon gauge is much more delicate, but, in this case, its indications are erroneous, unless proper allowance be made for the friction of the leathers and the weight of the plunger. The most exact means yet employed for measuring the stress created by the plunger on the chain, consists in the use of a system of balanced levers, according to the plan adopted at H.M. Woolwich and Portsmouth dockyards, and by Messrs. Brown & Lenox. The press at Woolwich is also furnished with a weighted valve, according to the plan just mentioned, and in addition to the system of levers. The lever scale is perfectly sensible to a few pounds, but the valve scale will scarcely move with a load of two tons, and it is less and less sensible as the loads increase. The balanced levers are perfectly accurate, but the apparatus is rather expensive. At the last Worcester Show of the Royal Agricultural Society, a certain apparatus (not patented) was exhibited for testing the draft of Fowler's six-furrow steam plough, and it appears to me that a modification of this dynamometer might be employed for registering the stress on a cable. It consisted essentially of a cylinder, and a piston, on one side of which was a volume of water in communication with a Bourdon gauge. The water was enclosed in an elastic diaphragm, fixed to the piston and to the cover, and the gauge was necessarily marked according to the results given by weights gradually applied. By shrinking rings on the outside, or by straining on a coil of wire, the cylinder could be made to stand any amount of pressure required, and, if adjusted with the cross

shackle pins at the opposite ends, at right angles to each other, in order to prevent any torsion, and also by the adoption of other simple means, such as the use of steel, that will occur to those now present, a light instrument of probably very great delicacy would be obtained.

When a long length of cable, say of seventy-five fathoms, is being tested, there is another influence that will, in some cases, affect the result. If we take the comparatively light one-inch cable, we find that it weighs 58 lbs. per fathom, so that the whole length will weigh nearly two tons. The last link at each end will have to stand a down pull of nearly one ton in addition to the longitudinal stress of 18 tons. This, however, would probably be practically compensated by directing the hammer test more towards the centre portions of the cable, and the *vis viva* of each blow will be absorbed by the elasticity of the metal, the deflection of each link struck, and by the combined weight and resilience of a certain portion of the cable within the range of each blow. It may here be noticed that in testing the effect of impact on beams, Mr. Hodgkinson used a 4 lb leaden cushion in order to partially deaden the jar of the blow. In a leading article of one of the engineering journals, in May last year, giving an account of Lloyd's proving-house, it was proposed that "a falling weight, to be released by a trigger tripped by a long cord," should be employed instead of the hammer, in order to prevent any accident to the operative, through the flying of the cable or a chip of the cast iron cross-stays. This weight could be made to slide overhead in the same vertical plane as the cable; and by letting it fall from the heights determined for each diameter of cable, the *vis viva* employed could be measured with approximate accuracy. This would only be on a par with the plan adopted in numberless instances, as we have seen, by our scientific neighbors the French; and similar measures might perhaps be used to measure the blow required to carry out the fracture test.

The application of known impulsive force as a test is of the utmost value, more especially when, as with cables, the object tested will have to undergo such forces in practice. If some plan could be devised for easily and accurately submitting the whole length simultaneously to a sudden instead of a static load, this would be of great importance. In the meantime, the hammer-blows are the tests for the resilience of the cable. In doubtful cases Professor Daniell's acid test might be of value in examining the structure of the fractured sections of the two or three links that are usually broken up. A great number of experiments on the specific gravity of iron have shown that it would be dangerous to make deductions as to the qualities of a specimen of wrought iron worked by one metallurgical process, and to then apply these results to a bar produced by another mode—for instance, to campare in this way a rolled bar with a hammered bar. At the same time there is a remarkably close, though not perfect, correspondence between the specific gravity and the quality of the specimens. Mr. Kirkaldy found that the specific gravity of iron was even decreased by being much strained—at any rate by tension. It is very easy to obtain the specific gravity of any substance like iron; and whether the physical facts

that, 1st, the gravities of, for instance, No. 3 bars, bear a pretty constant relation to their qualities; and that, 2d, the specific gravity of wrought iron generally is diminished by tensional straining; and, 3d, that it is considerably increased by annealing, might be used in practice for testing the quality of the iron, or the deterioration through wear of a chain, is at least worth an inquiry.

The physical conditions involved in the construction, the use, and the testing of anchors, differ so materially from those of chain cables, that the two subjects must be separated in an examination of this kind. But there can be little doubt that a sound and general system of testing the mooring tackle of ships will bring about the same improvement in the quality of chain cables and anchors, as the trials at Shoeburyness have already effected in the quality of rolled plates; and the effect will indeed be produced by somewhat similar causes.

(To be Continued.)

For the Journal of the Franklin Institute.

Bars at the Entrance of Harbors on Tide-water. By D. S. Howard, C. & M. Eng.

The same general laws will apply to the formation of bars in tide-water, when formed by currents either at the mouths of rivers or by the tide at the entrance of bays, but most bars at the entrance of bays, when the bays are of a capacity to quiet the current of the streams entering them, or those receiving no streams, are formed by winds blowing parallel, or nearly so, with the coast, where material exists which may be removed by the waves, and still heavy enough to sink in the heaviest sea-water.

The conduct of water, under the influence of wind, is well known to the most casual observer, so far as it may be seen on the surface, but the under currents in the constitution of waves are not so well understood. In deep water, with a steady constant wind, we see the swells large and smooth, with no disposition to translate anything on their surface horizontally in any direction. These waves continue in unobstructed sea room, far beyond the direct influence of the wind; they then become what are called "dead swells," and continue in that character until they approach shoal water, when the under current becomes quickened by contraction, and the disposition of the wave is determined by the form of the bottom. If a gradual smooth shoal, until they reach the shore, the waves become shorter and more curved on the back, as the under current is more and more increased by contraction, until they break, forming a crest-like appearance, (they are then called breakers,) the surface water sometimes rolling back and the remainder rolling forward until they reach an elevation on the shore indicated by the perpendicular height and the momentum of the swell.

These breakers show more or less distinctly the different currents constituting waves, which are, undoubtedly, generated in the incipient sea, continued through the dead swell, and fully developed in the

breaker, showing conclusively the formation of a vertical eddy under every wave. When the winds are parallel to the coast, these vertical eddies at certain depths of water, indicated by the force of the wind and the size of the wave, take up whatever loose material may be in their way, and deposit again in the direction of the motion of the wave, at a distance of one wave from the other, each one taking it up and depositing it as before, until it reaches a depression in the bottom like the entrance to a bay or harbor, where it may rest out of the reach of that depth of wave.

There is a difference between the vertical eddies under waves and those formed by a current rising from deep to shoal water, such as form bars in rivers, and sometimes at the entrance to harbors in tide-water; these are stationary as a whole, like their cause, while those partake of the same movements of their parent waves, and require a deeper place for a permanent deposit, while these require a shoaler place, and must also be furnished with material by an extended current, like that of a river, while those pick up their own material wherever the trough of the sea approaches the bottom sufficiently to create the necessary force to the under current.

There are few outer bars now formed in tide-water, not traceable to the action of waves, except such as that at the mouth of the Mississippi. There are also few rivers in this age of the world, with anything like a fixed regimen, emptying into tide-water, that have not extended their delta so far into the ocean as to diminish the force of their currents so much as to render them incapable of conveying any material of a size and density of substance sufficient to sink in sea-water.

The immense amount of water passing out at the mouths of the Mississippi river at the time of freshets, is such that the salt-water is forced out beyond the bar, allowing the deposits to be made in fresh water, by a vertical eddy formed by the lower current of the river rising from deep water a short distance above, where they remain as long as the freshet is sufficient to keep the heavier salt water from the bar. When the high water subsides sufficiently, the heavier salt-water of the Gulf finds its way, through the help of the wind and tide, into the river so far as to float away the bar, partly in the form of what are called "mud lumps," which arise to the surface in large bodies, and are driven ashore, or out to sea, as the stronger winds or tides may prevail, and partly by a semi-fluid, formed by the mingling of the salt-water with the material of the bar, both being so near the same weight as to render the deposits subject to similar laws of translation, consequently are gradually disposed of along the coast by the winds and waves, transferring shoals into islands and peninsulas, forming the bays, which abound on the Gulf coast.

It is well known that there is plenty of water over the bar at the mouth of the Mississippi except in high freshets, when the salt water is forced beyond the bar. Still, it is also well known, that there is no time, even in the lowest water, when there is not a large amount of material brought down from the turbulent Missouri, of the same

character as that which comes in with the freshets, but no deposit is made on the bar; some may settle in deep fresh water above, to be swept out by the lower current of the freshets, and remain on the bar until floated away by the salt-water when the freshets subside; then the heaviest salt-water forces its way under the fresh far above the bar, in flood-tide, loosening up the deposits and preparing them for removal by the ebb-tide, assisted by the flow of the river, which determines their destination outwards into the Gulf.

It would seem, from the most reliable facts which may be gathered from experience and observation, that the only true way to prevent bars from forming at the entrance to harbors in tide-waters, is to construct piers on each side of the channel, near enough together to prevent the coasting winds from creating a sea from one to the other; then dredge or scour out the channel to a uniform depth, so that, if there should be any silt or sediment floating in or out of the harbor by the tide or river current, there would be no tendency to deposit by the action of vertical whirls or eddies created by elevations or depressions in the bottom.

This has been the most approved plan by the most eminent engineers, but the difficulty in many cases in obtaining stone for the construction of piers has led to the conclusion, that nearly all of our harbors south of New York, and many others on the Atlantic coast, are out of the pale of improvement; but, in my opinion, a few simple experiments, properly conducted, would prove that the prime ingredient of a concrete, abundantly sufficient for the purpose, can be found wherever bars are formed, and procured at a cost that would settle the question of expense in favor of the practical improvement of any harbor on the Atlantic coast.

Elwood, N. J., Sept. 1, 1864.

On the Machinery employed in sinking Artesian Wells on the Continent. By G. R. Burnell, F.G.S.

It was stated that the extraordinary depth of some borings lately made for the purpose of obtaining an Artesian supply of water had led to great changes in the well-borers' art, and to the introduction of various mechanical processes, and of modifications of machinery, into the merits of which it was desirable to inquire.

The first well of this kind was that at the Abbatoir of Grenelle. This was sunk, after eight years incessant labor, a total depth of 1797½ ft. (English,) and gave rise to many inventions for the purpose of facilitating the progress of the works, for removing the broken tools, for the introduction of the pipes, and for carrying on observations at various depths from the surface. Subsequently many similar wells were sunk on the Continent, particularly in the Rhine provinces, but they were all of small diameter. The German engineers introduced important modifications in the tools. Æneyhausen made the striking part used for comminuting the rock to slide, so as to fall always through a certain distance, and thus avoid a jar. Kind had already applied

his system to large excavations for winning coal mines, when he was entrusted by the Municipal Councils of Paris, with the execution of the well at Passy. This was to have a diameter of one metre (3 feet $3\frac{7}{8}$ in.), that of Grenelle being only 20 centimetres (about 8 inches). The difficulties encountered in carrying the excavation through the clays of the upper series were so serious that six years and nine months were occupied in reaching the water-bearing stratum, which was ultimately attained at a depth of 1913 ft. 10 ins. from the surface, when the yield was 3,349,200 gallons per day of twenty-four hours, subsequently increased to 5,582,000 gallons and then continued at 3,795,000 gallons per day. The total cost of the well was £40,000. It was lined with solid masonry for a depth of 150 feet; then wood and iron tubing was introduced to 1804 feet from the surface, and below that there was a length of copper pipe pierced with holes.

The results of this well had been so satisfactory, as regarded the quantity of water, that the authorities had decided to execute at once two additional Artesian wells, and there were rumors that two others were contemplated.

There were three different systems of well-boring, mostly dependent on the nature of the tools: the Chinese, or M. Fauvelle's; the French, or rather the usual well-borers' plan; and M. Kind's. In the first, the motion given to the tool in rotation was simply derived from the resistance that a rope would excercise to an effort of torsion, and therefore the limits of application of the system were only such as would allow the tool to be safely acted upon. Besides, a considerable quantity of water was required to clear out the boring, so that this plan had been almost universally abandoned. In the ordinary system of well-boring, the weight of the tools and of the solid iron rods became so great when the excavation was deep, that there was considerable difficulty in transmitting the blow of the tool, in consequence of the vibration produced in the long rod, or in consequence of the torsion. Hollow rods filled with cork, and M. Æneyhausen's joint, which permitted the tool to fall freely and through the same height every time it was released, were now employed. M. Kind adopted both these modifications, and in the well of Passy he substituted oak rods for iron ones, as being lighter and more easily counterbalanced in water. The products of the excavation were still most frequently removed by augers and chisels; and all the processes hitherto practised were considered to be more or less defective, as in every case the comminuting tool had to be withdrawn. In the well at Passy, M. Kind employed a trepan to comminute the rock; it weighed 1 ton 16 cwt. and fell through 2 feet. This tool was composed of two principal pieces—the frame and the arms—both of wrought iron, but the teeth of the cutting part were of cast steel. The frame had at the bottom a series of holes slightly conical, into which the teeth were inserted, and were tightly wedged up. These teeth were placed with their cutting edge on the longitudinal axis of the frame that received them; and at the extremity of the latter there were formed two heads forged out of the same piece with the body of the tool, which also carried two teeth placed in the

same direction as the others, but which were made of double the width of the latter in order to render this part of the tool more powerful. It was by increasing the dimensions of these end-teeth that the diameter of the boring could be augmented, so as to compensate for the diminution of the clear space by the tubing that it might be necessary to introduce in traversing strata disposed to fall in, or to allow the waters from below to escape at an intermediate level. Above the lower part of the frame of the trepan was a second piece, composed of two parts bolted together, and made to support the lower portion of the frame. This part of the machinery also carried two teeth at its extremities, which served to guide the tool in its descent, and to work off the asperities that might be left by the lower portion of the trepan. Above this again were the guides of the machinery properly speaking, consisting of two pieces of wrought iron arranged in the form of a cross, with the ends turned up, so as to preserve the machinery perfectly vertical in its movements, by pressing against the sides of the boring already executed. These pieces were independent of the blades of the trepan, and might be moved closer to it or further away from it as might be desired. The stem and arms were, lastly, terminated by a single piece of wrought iron, which was joined to the frame by a kind of saddle joint, and was kept in its place by means of keys and wedges. The whole of the trepan was finally jointed to the great rods, that communicated the motion from the surface, by means of a screwed coupling, formed below the part of the tool that bore the joint which permitted the free fall of the cutting part, and united the top of the arms and frame and the rod.

Proc. Inst. Civil Eng., May 24, 1864.

The Adhesion of Krupp's Patent Steel Tyres.

From the London Mechanics' Magazine, June, 1864.

From a recent return made by the machinery department of the Chicago, Burlington & Quincy railway, we glean the following interesting facts respecting the traction of Krupp's Patent Steel Tyres. Inasmuch as some locomotive superintendents have had their doubts as to the adhesion of this tyre, the data contained in this report becomes the more important and valuable:

It appears that a set of Krupp's Tyres have been running under engine "34" thirteen months. The weight of the engine is 34 tons, and it is used in the heaviest goods service. During ten days of the past winter, this engine was behind a snow-plough with three engines back of it. It was mainly relied on for backing out of a snow-bank, and its wheels were found to slip less and to bite stronger than those tyred with iron. The engine was watched very closely through this severe test, which is regarded by the company as conclusive of the perfect traction of the tyre.

The wear in thirteen months is very slight, and the tyre is considered good for another year before going in for the first turning. The tyre retains its perfect circularity.

The toughness of the Krupp steel affords an easy solution of this traction problem, which has caused much speculation in the minds of many railway managers.

For the Journal of the Franklin Institute.

General Problem of Trussed Girders. By De Volson Wood, Prof. of C. E., University of Michigan.

I wish in the following article to show how to investigate truss bridges from the fundamental equations of statics.

The Problem. *It is proposed to find the strains upon the several pieces which form a truss when acted upon by any system of forces.*

The demonstration is founded upon the following proposition, which follows directly from statics.

Proposition. *If a frame of any form or combination be acted upon by any system of external forces, and the frame be completely divided into two parts by an ideal plane, the strains which act along the bars which are divided by the plane, are in equilibrium with the external forces which act upon the part of the frame on either side of the intersecting surface.*

We may readily conceive that the forces on one side of the ideal plane, hold the frame for the others to act, and the pieces transmit the forces (strains) from one part to the other; hence we may conceive that the forces on *either* side of the plane cause the strains.

We have then only to substitute forces for the strains and treat of them according to the principles of statics.

Let fig. 1 represent the combination of bars (or pieces). Take the origin of co-ordinates at O anywhere in the plane of section.

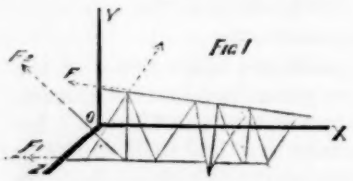


Fig 1

Let Y, be vertical and positive upwards.

X, horizontal and positive on the side of the section that the forces are considered.

Z, also horizontal and positive to the right of X as we look from Y towards O.

The precautions in regard to the signs are only necessary to enable us to use the same sign throughout the discussion and the applications.

Let P, P_1, P_2, &c., be the applied forces,

a, *b*, and *c*, the angles which P make with X, Y, and Z, respectively; and the same letters with subscripts, the corresponding angles of P_1, P_2, &c.

F, F_1, F_2, &c., the strains on the bars which are intersected by the plane.

α, β, and, γ the angles of F with X, Y, and Z, respectively, and the same letters with subscripts for the corresponding angles of F_1, F_2, &c.,

ΣX the sum of all the X components of the forces; similarly for ΣY and ΣZ.

Now conceive that the frame is divided and one part removed, and that in the plane section forces are substituted which will cause the same strains on the intersected bars that existed before the section was made; it is evident that the forces are in equilibrium the same as before one part was removed.

As the forces are in equilibrium we have

the forces:
$$\Sigma X = 0$$
$$\Sigma Y = 0$$
$$\Sigma Z = 0$$

and the couples:
$$\Sigma Z y - \Sigma Y z = 0$$
$$\Sigma X z - \Sigma Z x = 0$$
$$\Sigma Y x - \Sigma X y = 0$$

which by development gives—

the forces:
$$\left.\begin{aligned} P\cos a + P_1\cos a_1 + \&c. + F\cos\alpha + F_1\cos\alpha_1 + \&c. = 0 \\ P\cos b + P_1\cos b_1 + \&c. + F\cos\beta + F_1\cos\beta_1 + \&c. = 0 \\ P\cos c + P_1\cos c_1 + \&c. + F\cos\gamma + F_1\cos\gamma_1 + \&c. = 0 \end{aligned}\right\} (1)$$

and the couples

$$\left.\begin{aligned} \text{around X}, \Sigma P(y\cos c - z\cos b) + \Sigma F(y\cos\gamma - z\cos\beta) = 0 \\ \text{“ Y}, \Sigma P(z\cos a - x\cos c) + \Sigma F(z\cos\alpha - x\cos\gamma) = 0 \\ \text{“ Z}, \Sigma P(x\cos b - y\cos a) + \Sigma F(x\cos\beta - y\cos\alpha) = 0 \end{aligned}\right\} \quad . \quad (2)$$

Equation (1) may be written more concisely, thus:

$$\left.\begin{aligned} \Sigma P\cos a + \Sigma F\cos\alpha = 0 \\ \Sigma P\cos b + \Sigma F\cos\beta = 0 \\ \Sigma P\cos c + \Sigma F\cos\gamma = 0 \end{aligned}\right\} \quad . \quad . \quad . \quad (3)$$

Discussion of equations (1) *and* (2).

1°. In the earlier part of the discussion we will suppose that the applied forces, P, P_1, &c., are given, and hence their components may be readily computed. Considering them as known quantities and placing them in the second member we have—

the forces:
$$\left.\begin{aligned} \Sigma F\cos\alpha = -\Sigma P\cos a \\ \Sigma F\cos\beta = -\Sigma P\cos b \\ \Sigma F\cos\gamma = -\Sigma P\cos c \end{aligned}\right\} \quad . \quad . \quad . \quad (4)$$

and the couples;

$$\left.\begin{aligned} \text{around X}, \Sigma F(y\cos\gamma - z\cos\beta) = -\Sigma P(y\cos c - z\cos b) \\ \text{“ Y}, \Sigma F(z\cos\alpha - x\cos\gamma) = -\Sigma P(z\cos a - x\cos c) \\ \text{“ Z}, \Sigma F(x\cos\beta - y\cos\alpha) = -\Sigma P(x\cos b - y\cos a) \end{aligned}\right\} \quad . \quad (5)$$

2°. In (4) and (5) there are six independent equations; hence the problem is determinate, and may be solved by elimination, if the plane

intersects six bars or less. If it intersects more than six it is indeterminate, unless conditions are established among the strains.

3°. If the frame has an axis, let X coincide with or be parallel to it, then we call

$\Sigma P \cos a$ the DIRECT STRESS, $\Sigma P \cos b$ and $\Sigma P \cos c$ the total SHEARING STRESS, $\Sigma P (\cos b - y \cos a)$ the BENDING COUPLE, and the other two the TWISTING COUPLES.

Practical examples of such a general character very rarely occur.

4°. The strains upon the bars depend upon their position, and not upon the trussing between them and either end.

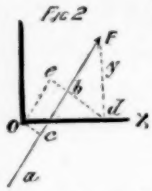
5°. Let all the forces be in the plane XY. Then will $c = 90°$, $\gamma = 90°$, $(a+b) = 90°$, and $(a+\beta) = 90°$, and equations (4) and (5) become

$$\left.\begin{aligned} \Sigma F \cos a &= -\Sigma P \cos a \\ \Sigma F \cos \beta &= -\Sigma P \cos b \text{ also} = \Sigma F \sin \beta \\ \Sigma F (x \cos \beta - y \cos a) &= -\Sigma P (x \cos b - y \cos a) \end{aligned}\right\} \quad (6)$$

in which the first is the *direct stress*, the second the *shearing stress*, and the third the *bending couple*.

The quantity $(x \cos \beta - y \cos a)$ is the length of the perpendicular let fall from the origin on F. For, let ab, fig. 2, be a bar along which F acts; O the origin.

The point of application of the force may be anywhere along ab; let it be at F, and draw Fd perpendicular to X; db and Oc perpendicular to ab, and Oe parallel to ab.



Then $Od = x$; $dF = y$, $bdF = a$, and $bFd = \beta$

$bd = y \cos a$, $ed = x \cos \beta$

$\therefore Oc = eb = ed - bd = x \cos \beta - y \cos a.$

Call this perpendicular q, and the second member Mz, and the equation becomes $\Sigma F q = -Mz$.

6°. From (6) we see that the problem is determinate when the plane section intersects three bars. If more, conditions must be established among the strains.

Suppose the plane intersects three pieces; viz: the upper and lower chords and a brace (or tie), and

let F = the strain on the upper chord;

F_1 = the strain on the lower chord;

F_2 = the strain on the brace;

a, a_1 and a_2, the corresponding angles with X,

q, q_1 and q_2, the lever arms of the strains.

Then (6) will become

$$\left.\begin{aligned} F \cos a + F_1 \cos a_1 + F_2 \cos a_2 &= -\Sigma P \cos a \\ F \sin a + F_1 \sin a_1 + F_2 \sin a_2 &= -\Sigma P \cos b \\ Fq \quad + F_1 q_1 \quad + F_2 q_2 \quad &= -\Sigma P (x \cos b - y \cos a) \end{aligned}\right\} \quad (7)$$

EXAMPLE. Suppose that while the frame ABC, fig. 3, is being put in position, it is acted upon by the three forces P, P_1, P_2.

Let the frame be 50 feet long and weigh 100 lbs. per foot.

Also P_1=200 lbs.; β_1=150° ∴ a_1=−60°

P_2=300 lbs.; β_2=170° ∴ a_2=−80°

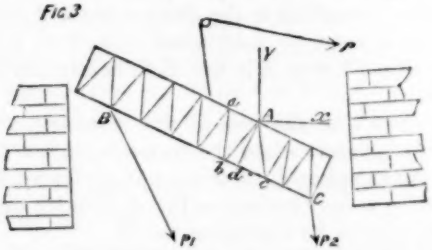
from which we may find P and a; for we have

$$\Sigma X = P\cos a + 200\cos 60° + 300\cos 80° = 0$$

$$\Sigma Y = P\sin a + 200\sin(-60°) + 300\sin(-80°) - 50\times 100 = 0$$

$$\therefore\ P = 5470 \text{ lbs};\ a = -88° 24\tfrac{1}{2}.$$

Fig 3



Now suppose we wish the strains on Aa, Ab, and bc. Intersect these bars by a vertical line or plane, and to simplify the case take the intersection infinitely near A. Let Aa and bc be parallel, and make an angle of −30° with X, and Ab an angle of +20°; Ad=q=6 feet, q and q_2 are zero.

According to the proposition, we may consider the forces on the right or left of A, and as there is but one external force on the right we will consider the forces on that side. Suppose there is 20 feet of frame on the right of A, and the center of gravity is 10 feet from Y; then will the moment be $20\times 100\times 10 = 20000$. Call the lever arm of P_2 15 feet. The strain on Aa is tensive, hence a=150°; on bc it is compression, hence a_1=−30°; on Ab it is compression, hence a_2=20°. Hence equation (7) become

$$F\cos 150° + F_1\cos(-30°) + F_2\cos 20° = -152{\cdot}08$$

$$F\sin 150° - F_1\sin 30° + F_2\sin 20° = 5468{\cdot}64$$

$$-F_1\times 6 = -(20000 + 300\times 15)$$

The signs of 6 F_1 is minus because it tends to turn the system from right to left—the signs of the other moments are positive because they tend to turn it in the opposite direction. From these we find

F=10862 lbs, F_1=4083½ lbs. and F_2=6082 lbs.

7°. Let all the forces be vertical: or, b=0° or 180,° $a=\pm 90°$ These in (7) give:

$$\left.\begin{aligned} F\cos a + F_1\cos a_1 + F_2\cos a_2 &= 0 \\ F\sin a + F_1\sin a_1 + F_2\sin a_2 &= -\Sigma \pm P \\ Fq \quad + F_1 q_1 \quad + F_2 q_2 \quad &= -\Sigma \pm P x \end{aligned}\right\} \qquad (8)$$

The plus sign belongs to forces which act vertically upward. It is well here to observe that if we consider tension as positive we *may* consider compression as negative; or we may consider both as positive and attribute the sign to the trigonometrical function—the same that we do with the applied forces—in which case a compression may be considered as a thrust whose direction is changed 180.° The hypotheses are essentially the same, although the latter is the one commonly used in establishing equations.

The first of equations (8) shows that the total compressive strain equals the total tensile ones.

The *shearing* stress, as found by the second of equations (7), *is greater on any vertical section when the longer part of the frame into which it is divided by the plane, is loaded and the shorter part unloaded.*

For if we conceive the longer part loaded—say from V, to the section, in fig. 5, and the remaining part unloaded—then will the total shearing stress on all vertical sections between the plane section and the other end be the same, and equal the reaction of the support, v; but if any load be now added to the shorter part, V will sustain a part of it while all of it must be subtracted from v to find the *shearing* stress on the section; hence it is less than before any load was placed on the shorter part.

It is easy to show that the sum of the moments, ΣPx, as given by the third of (7) is greatest when the frame is loaded throughout.

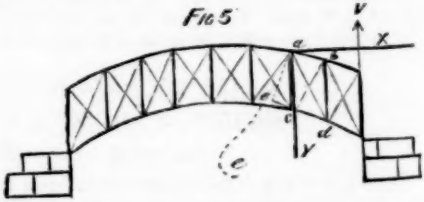
Equations (8) are applicable to trusses of all forms, but they may be reduced so as to be more convenient for particular cases.

8°. Let both chords be convex upwards.

If the space occupied by the truss be divided into trapezoids by vertical bars, as *ac* and *bd*, fig. 5, and the diagonal ones so arranged that only one shall act at a time—or what is equivalent, the diagonal bars shall be so arranged as to be subject to only one kind of strain, I shall call it the *panel* system. But if it be divided into triangles, as in fig. 4, I shall call it the *triangular* system.

Fig 4.

Fig 5



A bar makes the same angle with the axes that a line does which is drawn through the origin and parallel to the bar. We may assume any position of the bar to make positive angles with the axes, but after assuming the position we observe that a bar which inclines the opposite way makes a negative angle—but to be more specific we will assume, that when a line, or its prolongation, which is drawn through the origin and parallel to the bar, falls between $+X$ and $+Y$ the angle is positive —otherwise, it is negative. Still further, to avoid numerous negative angles which would otherwise occur in the position which we shall assume for the origin, I will make Y positive downwards. This will be equivalent to changing the signs of the vertical forces, and equations (8) become

$$\left.\begin{aligned} F\cos\alpha + F_1\cos\alpha_1 + F_2\cos\alpha_2 &= 0 \\ F\sin\alpha + F_1\sin\alpha_1 + F_2\sin\alpha_2 &= \Sigma\pm P \\ Fq \quad + F_1 q_1 \quad + F_2 q_2 \quad &= -\Sigma\pm Px \end{aligned}\right\} \quad . \quad (9)$$

in which plus P acts vertically upwards.

To determine the angle of the strains we conceive that forces are applied to the bars in the section which shall produce the same strains as before the section was made; then draw a line from the origin parallel to, and in, the direction of the action of the force, and it will make the same angle with the axes that the force does. We also observe that $+a$ is reckoned from $+x$ towards $+y$.

To prepare equations (9) for use, we will make a section at the right of, but indefinitely near to a, in fig. 5.

Let i = the angle which the upper chord makes with x.

i_1 = the angle which the lower chord makes with x.

θ = the angle which the brace $a\,d$ makes with y.

v = the vertical reaction of the support.

x_1 = the lever arm of v.

$h = ac$ = the vertical depth of the frame at the origin.

Σ_o^x p = the sum of all the loads between the origin and the end.

$\Sigma_o^x p x$ = the moments of all the loads between the origin and v.

Take the origin at a. The strain on ab is compressive, to produce which requires that f act from a towards b; hence $a=i$. f_1 must act at c, and towards e; hence $a_1=180°+i_1$; and f_2 acts from a towards d; hence $a_2=90-\theta$. $q=0$; $q_1=ae=h\cos i_1$; $q_2=0$. These values reduce (9) to

$$\left.\begin{aligned} f\cos i - f_1\cos i_1 + f_2\sin\theta &= 0 \\ f\sin i - f_1\sin i_1 + f_2\cos\theta &= v - \Sigma_o^{x_1} p \\ f_1 h\cos i_1 &= v x_1 - \Sigma_o\, p x \end{aligned}\right\} \quad . \quad . \quad (10)$$

For maximum *shearing* stress $\Sigma_o^x p = 0$; and we have

$$\left.\begin{aligned} f\cos i - f_1\cos i_1 + f_2\sin\theta &= 0 \\ f\sin i - f_1\sin i_1 + f_2\cos\theta &= v \\ f_1 h\cos i_1 &= v x_1 \end{aligned}\right\} \quad . \quad . \quad . \quad (11)$$

It must be observed that v in this case is not the same as in the preceding, but it must be computed from the given data.

Example. Suppose that in a bridge which is 120 feet long it is observed that at 30 feet from the end the upper chord is inclined 15° to the horizon; the lower chord 20,° the brace 45,° and the depth is 10 feet. It is required to find the strains upon these bars when there is an uniform load of one ton per foot over the whole length of the bridge: also the strains at the instant of maximum shearing, when it moves off without shock.

For the former part of the problem we use (10) and readily have

$$\left.\begin{aligned} f\cos 15° - f_1\cos 20° + f_2\sin 45° &= 0 \\ f\sin 15° - f_1\sin 20° + f_2\cos 45° &= 60 - 30 = 30 \\ 10\, f_1\cos 20° &= 60.30 - 30.15 = 1350. \end{aligned}\right\}$$

These solved give

$$f=77,\ f_1=143,\ \text{and}\ f_2=85\ \text{tons.}$$

The results are given to the nearest whole number.

For the maximum shearing, the load, according to the principles before stated, must extend over 90 feet. For this we find $v=\frac{90\cdot 45}{120}=33\frac{3}{4}$, which in (11) gives

$$\left.\begin{aligned} F\cos 15^\circ - F_1\cos 20^\circ - F_2\sin 45^\circ &= 0 \\ F\sin 15^\circ - F_1\sin 20^\circ + F_2\cos 45^\circ &= 33\tfrac{3}{4} \\ 10F_1\cos 20^\circ &= 1012\tfrac{1}{2} \end{aligned}\right\}$$

which being solved will give

$$F=43{\cdot}2,\ F_1=107{\cdot}6,\ F_2=84{\cdot}4 \text{ tons},$$

which compared with the preceding shows that the strain on the brace is nearly as great as when the whole bridge is loaded. We shall hereafter see that there are cases in which the strain on the braces is *greater* for maximum shearing than when the whole frame is loaded.

If *bc* is brought into action as a brace, it must be done by the loading between *b* and v.

(To be Continued.)

For the Journal of the Franklin Institute.

The Cinematics of the Slide Valve. By Albert Aston, U.S. Corps of Naval Engineers.

As experience has proved the slide valve, cutting off by lap, to be, of all devices, the best adapted to locomotive and screw-propeller engines, a set of reliable formulas, embracing all the practical conditions, for determining the proportions necessary for its proper action, will doubtless be acceptable to those engaged in its construction.

In the following discussion, strict mathematical accuracy is not aimed at, as that would only uselessly complicate the problem. The results given by the formulas, however, will, within the limits of practice, differ from the absolute truth only by insensible quantities.

The principal dimensions to be determined are the throw of the eccentric and the lap of the valve. For these we have given the length of stroke of the piston, or the length of the crank; the point of cutting off, and the width of opening of the steam-port.

The action of an eccentric is to be regarded as precisely the same as that of a crank, the centre of the eccentric corresponding to the centre of the pin of the crank. The distance from the centre of the shaft to the centre of the eccentric will be called, for convenience, the radius of the eccentric.

If a valve be without lead or lap, and the connecting-rod and eccentric-rod are supposed to be infinitely long, it will be in mid-position, or at half its stroke, when the piston is at the beginning of its stroke, its edges will coincide with the edges of the steam-ports, and its stroke will be exactly double the width of the port; but if lap on the steam side be given to the valve, its length will be correspondingly increased beyond the edges of the ports, consequently its stroke must be increased by exactly the increased length of the valve, and the centre of the eccentric must be revolved about the shaft from its mid-position until the extreme edge of the valve again coincides with the edge of the port, as the valve must be on the point of opening when the piston is at the commencement of its stroke.

It will be seen by inspecting the diagram, Plate III, that the valve will open while the centre of the eccentric passes through the arc *pd*, and that

Fig. 1.

Fig. 3.

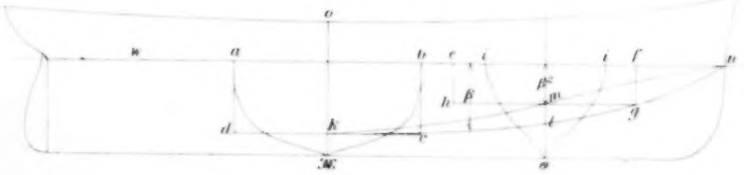


Fig. 2.

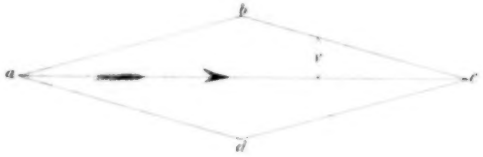


Fig. 4.

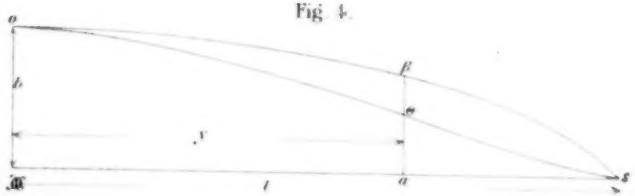
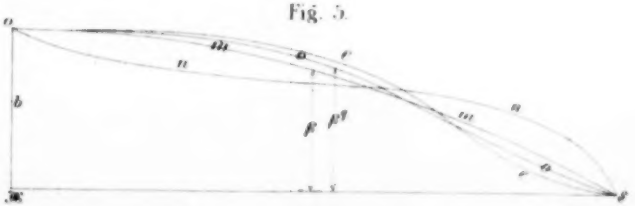


Fig. 5.



it will close while the same point passes through the equal arc dp', the points p and p' being equally distant from any point in the straight line AB; therefore, the versed-sine of the arc pd is equal to the width of opening of the steam-port. As the centre of the eccentric was revolved about the shaft through the arc mp on account of the lap, the distance through which the valve moved (which is equal to the lap) is equal to the sine of the arc mp, or, to the cosine of its complement, pd. If the centre of the eccentric should pass to the point p'', which is directly opposite the point p, the other end of the valve would be the point of opening.

Both the crank and eccentric being secured to the same shaft, it is evident they must pass through equal angles during any portion of a revolution. Thus, if the crank-pin pass from P to P′, the centre of the eccentric will pass from p to p', the angle PcP′ being equal to the angle pcp'. Therefore, if the valve be required to open and close while the crank-pin passes from P to P′, the centre of the eccentric must be secured at the point p when the crank-pin is at P, and the angle pcd equal to one-half of pcp', or one-half of PcP′.

To find the radius of the eccentric when the width of the steam-port and the angle PcP′ are given,

Let a = width of steam-port opening,
r = radius of eccentric,
β = angle PcP′ through which the crank has passed.

Then
$$a = r - r\cos\frac{\beta}{2}$$

$$a = r\left(1 - \cos\frac{\beta}{2}\right)$$

$$r = \frac{a}{1 - \cos\frac{\beta}{2}} \quad . \quad . \quad . \quad (1)$$

To find the value of $\cos\frac{\beta}{2}$ in terms of the stroke of piston and point of cutting off, let R = the length of the crank, L = the length of stroke to be accomplished with the full pressure of steam,

then
$$R\cos\beta = R - L.$$

$$\cos\beta = \frac{R - L}{R}$$

The formula for the cosine of half an arc is

$$\cos\frac{\beta}{2} = \sqrt{\frac{1 + \cos\beta}{2}}$$

$$\therefore \cos\frac{\beta}{2} = \sqrt{\frac{1 + \frac{R-L}{R}}{2}} = \sqrt{\frac{2R - L}{2R}}$$

Substituting this value in (1), we have

$$r - \frac{a}{1 - \sqrt{\frac{2R - L}{2R}}} \quad . \quad . \quad (2)$$

We have seen before, that the lap of the valve is equal to $r \cos \frac{\beta}{2}$ or, which is the same thing, $r - a$; therefore, if we represent it by x, we have

$$x = r - a \quad . \quad . \quad . \quad (3)$$

These values for the radius of the eccentric and the lap of the valve are true, provided the valve has no lead, and supposing the eccentric-rod to be always parallel to a line bisecting the angle $p\,c\,p'$. As the former is seldom, and the latter never the fact in practice, it will be necessary to investigate the effect of the derangement thus caused. It will be seen, however, that these two causes of derangement act in opposition to each other, the one tending to increase, and the other to diminish the radius of the eccentric; and, with the amount of lead usually employed, if the eccentric-rod has much angularity, they become nearly equal, thereby neutralizing each other. We will first consider the lead.

If the lead should be given by moving the eccentric around on the shaft to any point, q, the valve would still close when the centre of the eccentric arrived at the point p'; or, the steam would be cut off when the crank had passed through an angle equal to $q\,c\,p'$, which is less than the angle $p\,c\,p'$, or its equal $P\,c\,P'$, thus bringing the point of cutting off nearer to the commencement of the stroke of the piston. On the other hand, if the lead should be taken from the valve, the valve would not close until the centre of the eccentric had passed the point p' to some point, q', which would cause the crank to pass through an angle equal to $p\,c\,q'$, which is greater than $p\,c\,p'$, thereby bringing the point of cutting off nearer the *end* of the stroke of the piston.

As the lead is usually but a small portion of the stroke of the valve, it will be sufficiently accurate, for practical purposes, to consider the angle $p\,c\,q$ equal to the angle $p'\,c\,q'$, and to take one-half the lead from the lap of the valve and give the other half by moving the eccentric around the shaft to a point, u, midway between p and q. This can be done whatever may be the position of the point p.

As the valve will now be partly open when the centre of the eccentric is at the point p, or p', it is evident that the radius of the eccentric may be lessened so that the versed sine of the arcs $p\,c\,d$ or $d\,c\,p'$ plus the distance the valve is open, may be equal to the width of the steam opening, a. The distance the valve is open being one-half the lead, if we represent the lead by l, we have for the value of the versed sine of the arc $p\,d$,

$$a - \frac{l}{2}$$

The other cause of derangement, the obliquity of the eccentric-rod, causes the valve to open wider at one end than at the other, thus: Instead of the width of opening of the steam-port being equal to the versed sine of the arc $p\,d$, or to a, it will be equal to $s\,d$, or, a minus the versed sine of the arc $p\,s$, while at the other end of the valve it will be equal to $s'd'$. As the width of steam opening assumed is supposed to be as small as it properly can be, the value of r should be increased so that the distance $s\,d$ will give the proper opening. To this end, it will be necessary to add the versed sine of the arc $p\,s$ to that portion of r represented by a. Let this versed sine be represented by v, and we will then have for the value of the versed sine of the arc $p\,d$.

$$a-\frac{l}{2}+v.$$

which may be substituted for a in (2), when the formula will become

$$r=\frac{a-\frac{l}{2}+v.}{1-\sqrt{\frac{2\,\mathrm{R}-\mathrm{L}}{2\,\mathrm{R}}}} \qquad . \quad . \quad . \quad (4)$$

As v is a variable function of r, its value cannot be obtained in terms independent of that quantity; nor, is it a matter of sufficient consequence to require a rigorous mathematical determination. If the value of r be first found by equation (2), and v be computed from that value, the result given by (4) will be found sufficiently accurate. The length of eccentric-rods is seldom less than fifteen times r; and it is easily seen that these inaccuracies are practically imperceptible. If v is found to be nearly equal to $\frac{l}{2}$ both may be neglected.

To find the value of v, let r' equal the length of the eccentric-rod, and ϕ equal the angle formed by the rod with the line AB, then

$$v=r'-r'\cos\phi.$$

$$r'\cos\phi=\sqrt{r'^2-\left(r\sin\frac{\beta}{2}\right)^2},$$

$$\therefore\ v=r'-\sqrt{r'^2-\left(r\sin\frac{\beta}{2}\right)^2},$$

but

$$r\sin\frac{\beta}{2}=r\sqrt{\frac{1-\cos\beta}{2}}=r\sqrt{\frac{1-\frac{\mathrm{R}-\mathrm{L}}{\mathrm{R}}}{2}}=r\sqrt{\frac{\mathrm{L}}{2\,\mathrm{R}}},\ \text{and}$$

$$\left(r\sin\frac{\beta}{2}\right)^2=\frac{\mathrm{L}r^2}{2\,\mathrm{R}}$$

$$\therefore\ v=r'-\sqrt{r'^2-\frac{\mathrm{L}r^2}{2\,\mathrm{R}}} \qquad . \quad . \quad (5)$$

The equation for the lap will now become

$$x = r\cos\frac{\beta}{2} - \frac{l}{2} = r\sqrt{\frac{2R-L}{2R}} - \frac{l}{2} \quad . \quad (6)$$

which will be true whether $\frac{l}{2}$ and v are discarded from (4) or not.

The effect produced by the obliquity of the connecting-rod, is that of causing the valve to close sooner at one end than at the other, or, the steam will be cut off shorter at one end of the cylinder than at the other. The greater the obliquity of the rod, the greater will be the difference; but it may be partially remedied by giving the valve more lead at one end than at the other.

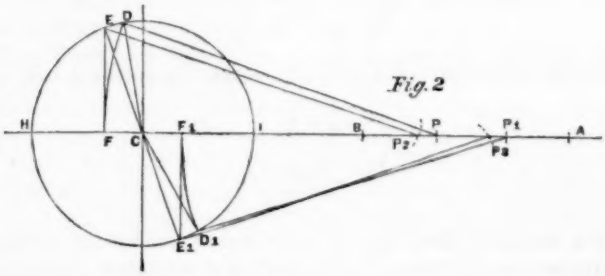


Fig. 2

In fig. 2, let AB represent the length of stroke of the piston or cross-head, and PP_1, points of cutting off equally distant from the ends A and B; then, taking the length of the connecting-rod, BH, as a radius, and the points P and P_1, as centres, describe the arcs FD and F_1D_1, which will cut the circumference described by the centre of the crank-pin in the points D and D_1. These points will represent the position of the crank-pin at the moment the steam is cut off; and it will be observed that the arc ID through which the pin passes on its outward stroke, is less than the arc HD_1 traversed by the pin on its inward stroke. Now, if the connecting-rod be supposed infinitely long, it will be always parallel to the line AH; the lines FD and F_1D_1, instead of being arcs, would be parallel straight lines; and, as they are equidistant from the centre of the circle described by the crank-pin and perpendicular to the line IH, they would be equal and would intercept equal arcs, IE and HE_1, of the circumference. If the points E and E_1 are assumed for the positions of the crank-pin at the time the steam is cut off, the cross-head must travel a distance greater than AP on its outward stroke, and a distance less than BP_1 on its inward stroke, for the arc IE is greater than the arc ID, and the arc HE_1 is less than the arc HD_1. Now, if we take the length of the connecting-rod, PF, as a radius, and the points E and E_1 as centres, and describe small arcs cutting the line AB in P_2 and P_3, these points will represent the positions of the cross-head, and the distances PP_2 and P_1P_3 will be equal; for the right-angled triangles FP_2E and $F_1P_3E_1$ are equal, and the difference between two adjacent sides of one, is equal to the difference between two corresponding adjacent

sides of the other. As AP_2 is equal to $AP+PP_2$, and BP_3 is equal to $BP_1-P_1P_3$, the distance AP or BP_1 is an arithmetical mean between the distances AP_2 and BP_3.

To find the distance PP_2 or P_1P_3, we have

$$PP_2=P_2E-\sqrt{P_2E^2-EF^2}.$$

The quantity $EF^2=(R\sin\alpha)^2=R^2-(R\cos\alpha)^2=R^2-(R-L)^2$.

Let the distance PP_2 be represented by s, and the length of the connecting-rod by C, then

$$s=C-\sqrt{C^2-[R^2-(R-L)^2]}, \quad . \quad . \quad (7)$$

which must be added to the mean distance of cutting off on the outward stroke and subtracted from it on the inward stroke.

If levers intervene between the eccentric and valve so that the stroke of the latter is increased or diminished, its dimensions must be regulated proportionately to the arms of the levers. This, however, will not apply to the Stephenson link, as the backing eccentric deranges the motion unless the axis of the forward eccentric-rod pin coincides with the axis of the link-pin. The usual method of attaching eccentric-rods to one side of the link, causes a slight derangement, but it is barely perceptible. The best way of finding the position of the link for intermediate points of cutting off, is as follows:—When the engines are set up and the valve-gear adjusted (the valve-chest cover being off if practicable), turn the shaft until the cross-head arrives at the point desired. Then move the link until the steam-port is just closed, and mark the position of the catch, on the guard or whatever other device may be adopted. Next, turn the shaft until the cross-head is in a corresponding position on the return stroke; move the link until the port is closed and mark the position of the catch. Cut the notch midway between the marks on the guard and proceed in the same manner for the other points of cutting off.

The only remaining question is that of the exhaust lap. If the exhaust lap was equal to the steam lap, the exhaust port would be closed at the same time as the steam-port, which would cause excessive cushioning. If, on the contrary, there was no exhaust lap, the exhaust-port would be open long before the steam-port, and, consequently, before the piston had arrived at the end of its stroke. The loss due to this too early release of the steam is more serious than that due to cushioning, for it is all a loss of power; whereas the compressed vapor partially or wholly fills the port and clearance which would, otherwise, have to be supplied with fresh steam from the boiler. In fact, if the expansion were carried down to the back-pressure, there would be no loss of economic effect by the cushioning, however excessive. The best relative proportion which these two losses should bear to each other, is, evidently, that in which the sum of the two would be a minimum. This could be easily determined by means of the differential calculus if the curve traced on the indicator card by the escaping steam, and which is dependent on the proportion between the valve-opening and the cylinder capacity, and the speed of the piston between the point

of release and the end of its stroke, could be known, and if the compressed vapor followed Mariotte's or any known law; but as the exhausting curve cannot be easily determined, and as the required condition is never fulfilled by the cushioning steam, accurate calculation is out of the question. It should be remembered, however, that the valve should open long enough before the end of the stroke to allow the piston to commence its return stroke with the maximum vacuum, and that the cushioning is not perceptible on the indicator card until some time after the exhaust-port is actually closed, owing to the rapid condensation of the compressed vapor; and were it not for the atmospheric air mixed with the steam in the cylinder, the cushion curve would be much less than it is actually found to be, and a much earlier closing of the exhaust would be practicable. The problem is also affected by the absolute amount of back pressure; but it is found, from the inspection of a large number of indicator cards, that the most satisfactory diagrams are obtained when the exhaust lap is about one-half the steam lap.

In order to find the distance of the piston from the end of the stroke at the time the communication with the condenser is cut off, it will be necessary to find the angle previously passed through by the crank or the centre of eccentric. This angle is composed of the angle, ucu', through which the centre of the eccentric passed before the steam was cut off, and the angle, $u'cw$, due to the difference between the steam and exhaust lap. As these angles are measured by their cosines on the line AB, we must suppose the angle ucw to be made up of the angles ucd and dcw. Let θ=the angle ucw; x'=the exhaust lap, and c=R—R cos θ=the distance of the piston from the end of the stroke,

$$\text{then } \cos\theta=\cos\left[\cos^{-1}\left(r\cos\frac{\beta}{2}-\frac{l}{2}-(x-x')\right)+\cos^{-1}\left(r\cos\frac{\beta}{2}+\frac{l}{2}\right)\right]$$

but $r\cos\frac{\beta}{2}-\frac{l}{2}$ = the lap on the steam side = x;

$$\therefore\ \cos\theta=\cos\left(\cos^{-1}\frac{x'}{r}+\cos^{-1}\frac{x+l}{r}\right) \text{ and}$$

$$c=\text{R—R}\cos\left(\cos^{-1}\frac{x'}{r}+\cos^{-1}\frac{x+l}{r}\right) \quad . \quad . \quad (8)$$

It will be observed, that if the quantity $\frac{x'}{r}$ is negative, the arc of which it is the cosine must be subtracted from 180°.

To find the distance of the piston from the end of the stroke at the time the steam is allowed to escape into the condenser, let θ' = the angle passed through by the crank, and e=R—R cos θ' = the required distance. The angle θ' will be equal to 180° minus the angle, $w'cu''$, due to the difference between the steam and exhaust lap, and we may suppose it to be composed of the three angles ucd, BcD=90°, and Dcu'', then

$$\cos\theta'=\cos\left[\sin^{-1}\left(r\cos\frac{\beta}{2}-\frac{l}{2}-(x-x')\right)+90°+\cos^{-1}\left(r\cos\frac{\beta}{2}+\frac{l}{2}\right)\right]$$

or, substituting the value of $r \cos \frac{\beta}{2} - \frac{l}{2}$ as before,

$$\cos \vartheta' = \cos\left(\sin^{-1}\frac{x'}{r} + 90° + \cos^{-1}\frac{x+l}{r}\right)$$

and

$$e = \text{R} - \text{R}\cos\left(\sin^{-1}\frac{x'}{r} + 90° + \cos^{-1}\frac{x+l}{r}\right) \quad . \quad . \quad (9)$$

These two values, c and e will give the mean points of cushioning and exhausting. As the obliquity of the eccentric-rod will slightly affect these values, the result will be still more accurate, if, in (7), we subtract v from x' for the outward stroke and add v to x' for the inward stroke, and in (8) add v to x' for the outward stroke and subtract it for the inward stroke. The reason for this will become apparent by inspecting the diagram. The equations will now become

$$c = \left\{\begin{array}{l} \text{R} - \text{R}\cos\left(\cos^{-1}\frac{x'-v}{r} + \cos^{-1}\frac{x+l}{r}\right) \text{outward stroke} \\ \text{R} - \text{R}\cos\left(\cos^{-1}\frac{x'+v}{r} + \cos^{-1}\frac{x+l}{r}\right) \text{inward stroke,} \end{array}\right\} \quad . \quad (10)$$

and

$$e = \left\{\begin{array}{l} \text{R} - \text{R}\cos\left(\sin^{-1}\frac{x'+v}{r} + 90° + \cos^{-1}\frac{x+l}{r}\right) \text{outward stroke} \\ \text{R} - \text{R}\cos\left(\sin^{-1}\frac{x'-v}{r} + 90° + \cos^{-1}\frac{x+l}{r}\right) \text{inward stroke} \end{array}\right\} \quad (11)$$

By substituting the value of $2\text{R} - c$ or $2\text{R} - e$ for L in (7), the value of s may be found for c or e; then $c - s$ and $e - s$ will be the true points for the outward stroke, and $c + s$ and $e + s$ the true points for the inward stroke.

The manner of proceeding to design a slide-valve will then be as follows:—

1st, After having determined the width of the steam-port opening and the point of cutting off, find the approximate radius of the eccentric by (2).

2d, Determine, as nearly as possible, the length of the eccentric-rod, and find by (5) the value of the versed sine of the angle which it forms with the horizontal line when the valve is on the point of opening.

3d, Assume the amount of lead desired, and then find the true radius of the eccentric by (4).

4th, Find the lap by (6).

5th, Assume the amount of exhaust lap, and determine the mean points cushioning and exhausting by (8) and (9). If these are not satisfactory, the exhaust lap may be varied until they are.

If it is desired to construct an indicator card, or if the actual points of cutting off, cushioning and exhausting are required for each stroke, the values of c and e can be determined from (10) and (11), and the

variation caused by the obliquity of the connecting-rod can be calculated from (7) by substituting for L the previously found mean distance of both strokes, passed over by the piston in reaching the point in question. These variations must be *added* to the distances from the *commencement* of the stroke, already found, for the outward stroke, and *subtracted* for the inward stroke.

Equations (8), (9), (10), and (11) might be transformed so that they could be worked without the aid of a table of natural sines, but they are now in their simplest form.

The diagram shows a method of ascertaining all the required values geometrically, after the radius of the eccentric and the lap of the valve are found.

It is practically desirable to keep the stroke of the valve as small as possible, and it is principally for this reason that the obliquity of the eccentric-rod has been taken into account. For the same purpose, the steam-ports should be made as narrow as possible, the requisite area being made up by length. This will cause more resistance to the passage of the steam, but the area can be slightly increased to compensate it.

As the steam is not required to enter the cylinder as quickly as it should leave it, the steam side of the valve should only uncover about three-fourths of the width of the port. The exhaust side should always give the full opening, which it will do if the exhaust lap is properly proportioned to the steam lap. The *opening* of the port on the steam side of the valve, is what must be used with the foregoing formulas. A good rule for finding the area, in square inches, of the steam-port, is, to multiply the square of the diameter of the cylinder, in inches, by the velocity of the piston in feet per minute, and divide by 4000.

It might, also, be observed, that the length of the link, from centre to centre, should be at least three times the stroke of the valve, and that the best radius for its centre line is the distance from the centre of the pin to the centre of the eccentric.

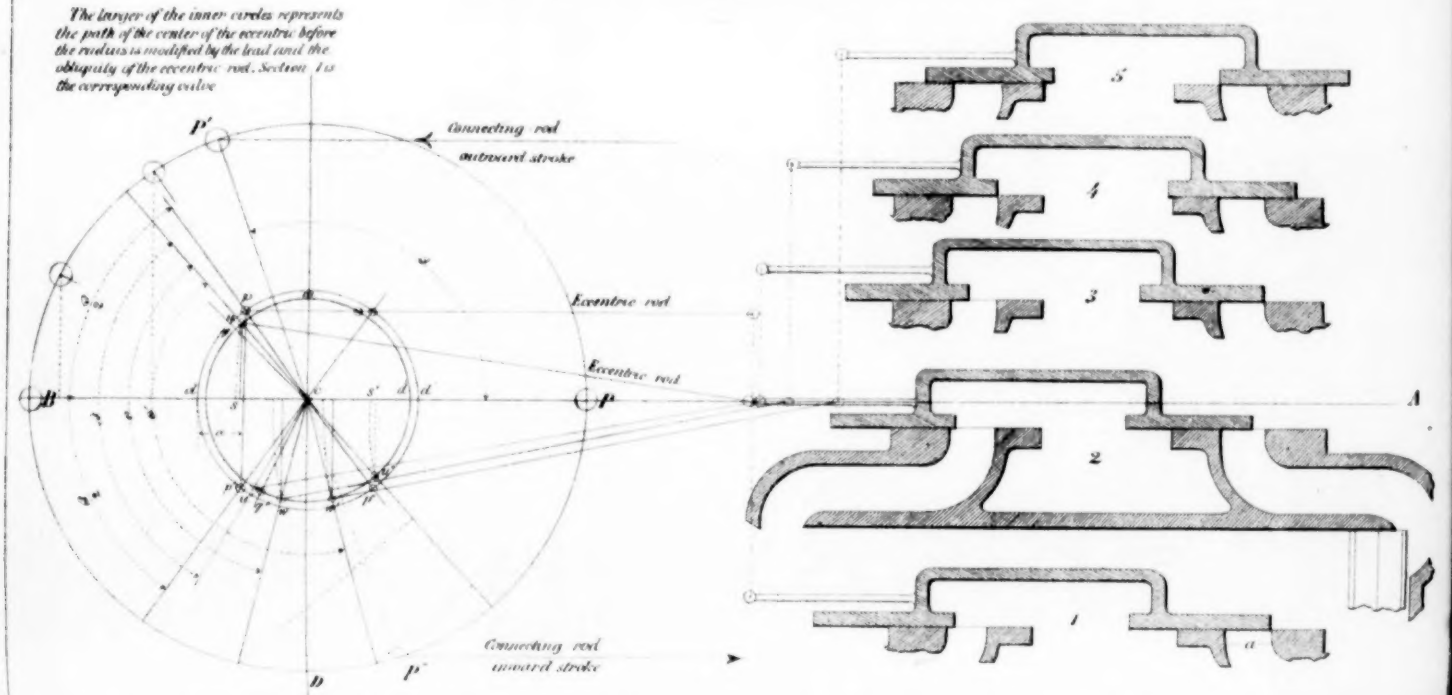
To show more plainly the method of computation, it is deemed expedient to subjoin the following

EXAMPLE. The stroke of the steam piston of an engine is 12 inches; the mean point of cutting off 8 inches from the commencement; the width of the steam-port $\frac{1}{2}$-inch; the length of the connecting-rod 30 inches; the lead $\frac{1}{8}$-inch; the length of the eccentric-rod 20 inches, and the proposed exhaust lap one-half the steam lap. Required the radius of the eccentric; the lap of the valve; and the points of cutting off, cushioning, and exhausting, for both strokes of the piston.

$$\frac{\cdot 5}{1-\sqrt{\frac{12-8}{12}}} = 1{\cdot}183 = \text{approximate radius of eccentric,} \quad . \quad (2)$$

$$20-\sqrt{20^2-\frac{8\times 1{\cdot}183^2}{12}} = \cdot 024 = \text{versed sine of arc of eccentric-rod} \quad (5)$$

DIAGRAM OF SLIDE VALVE MOTION



$$\frac{\cdot5-\cdot0625+\cdot024}{1-\sqrt{\frac{12-8}{12}}} = 1\cdot0919 = \text{true radius of eccentric,} \quad . \quad . \quad (4)$$

$$1\cdot0919\sqrt{\frac{12-8}{12}} - \cdot0625 = \cdot5679 = \text{lap,} \quad . \quad . \quad . \quad (6)$$

$$6-6\cos\left(\cos^{-1}\frac{\cdot2839-\cdot024}{1\cdot0919}+\cos^{-1}\frac{\cdot5679+\cdot125}{1\cdot0919}\right)=6-6\cos$$

$(76^\circ\ 14' + 50^\circ\ 37') = 6-6\cos 126^\circ\ 51' = 6-6\times\cdot5997 = 2\cdot4 =$ cushion for outward stroke independent of connecting-rod, . (10)

$$6-6\cos\left(\cos^{-1}\frac{\cdot2839+\cdot024}{1\cdot0919}+\cos^{-1}\frac{\cdot5679+\cdot125}{1\cdot0919}\right)=6-6\cos(73^\circ\ 37'$$

$+50^\circ\ 37') = 6-6\cos 124^\circ\ 14' = 6-6\times\cdot5625 = 2\cdot62 =$ cushion for inward stroke independent of connecting-rod, . . (10)

$$6-6\cos\left(\sin^{-1}\frac{\cdot2839+\cdot024}{1\cdot0919}+90^\circ+\cos^{-1}\frac{\cdot5679+\cdot125}{1\cdot0919}\right)=6-6\cos(16^\circ$$

$23'+90^\circ+50^\circ\ 37') = 6-6\cos 157^\circ = 6-6\times\cdot9205 = \cdot477 =$ exhaust for outward stroke independent of connecting-rod, . . (11)

$$6-6\cos\left(\sin^{-1}\frac{\cdot2839-\cdot024}{1\cdot0919}+90^\circ+\cos^{-1}\frac{\cdot5679+\cdot125}{1\cdot0919}\right)=6-6\cos$$

$(13^\circ\ 46'+90^\circ+50^\circ\ 37') = 6-6\cos 154^\circ\ 23' = 6-6\times\cdot9017 = \cdot589 =$ exhaust for inward stroke independent of connecting-rod, . (11)

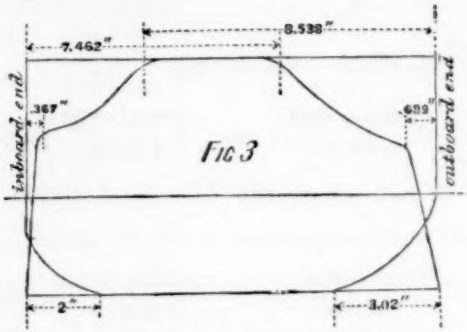
$30-\sqrt{30^2-[6^2-(6-8)^2]} = \cdot538 =$ variation in cut-off caused by connecting-rod, ... (7)

$30-\sqrt{30^2-[6^2-(6-9\cdot49)^2]} = \cdot4 =$ variation in cushion caused by connecting-rod, ... (7)

$30-\sqrt{30^2-[6^2-(6-11\cdot47)^2]} = \cdot11 =$ variation in exhaust caused by connecting-rod, ... (7)

ANSWER:—Radius of eccentric, 1·09 inch; lap of valve, ·56 inch; point of cutting off on outward stroke, $8+\cdot538 = 8\cdot538$ inches from commencement; point of cutting off on inward stroke, $8-\cdot538 = 7\cdot462$ inches from commencement; cushion on outward stroke, $2\cdot4-\cdot4 = 2$ inches from end; cushion on inward stroke, $2\cdot62+\cdot4 = 3\cdot02$ inches from end; exhaust on outward stroke, $\cdot477-\cdot11 = \cdot367$ inch from end; exhaust on inward stroke, $\cdot589+\cdot11 = \cdot699$ inch from end.

Fig. 3, constructed from these dimensions, represents the indicator diagram that would be given by an engine having proportions according to this example.



These computations can be made in an hour, and the results will be more accurate than can be obtained by an expensive wooden model.

Navy Department, Washington, Sept. 5th, 1864.

For the Journal of the Franklin Institute.

The Tunnel to Supply the City of Chicago with Water.

MR. EDITOR:—

I had the pleasure a few days since of visiting the Chicago Tunnel, which is intended to supply the city with water. The public may be pleased to know that it is progressing very favorably. I descended the land shaft and passed along the tunnel about 600 feet, to where the miners were at work. The earth which they were removing was a very stiff clay. The soil thus far has been the very best kind for securing permanency of the work. The tunnel is nearly circular, the horizontal diameter being 5 feet and the vertical 5 feet 2 inches. It will be two miles long when completed. The vertical shaft is nearly seventy feet deep. Thirty feet of the upper end is protected by cast iron cylinders which are seven feet in diameter. The remaining forty feet passes through clay and is bricked.

When worked to its utmost capacity it is thought that it might supply 1,000,000 inhabitants. According to contract it is to be completed by November 1st, 1865. D. V. W.

MECHANICS, PHYSICS, AND CHEMISTRY.

Proceedings of the Association for the Prevention of Steam Boiler Explosions, Manchester.

[Report of Chief Engineer, May 2, 1864.]

From the Lond. Practical Mechanic's Journal, May, 1864.

During the last month 311 engines have been examined and 418 boilers, 20 of the latter being examined specially, and 4 of them tested with hydraulic pressure. Of the 418 boiler examinations, 333 have

been external, 11 internal, and 74 thorough. In the boilers examined, 255 defects have been discovered, 4 of them being dangerous. Details of these will be found in the following statement.

Furnaces out of shape 4 (1 dangerous); fracture 6 (2 dangerous); blistered plates 6; corrosion 24; grooving-internal 5; external 6; feed apparatus out of order, 6; water gauges ditto, 14; blow-out apparatus ditto, 16; fusible plugs ditto, 1; safety valve ditto, 2; pressure gauges ditto, 18; without water gauges ditto, 3; ditto pressure gauges, 39; blow-out apparatus, 35; ditto feed back pressure valves, 68; cases of over-pressure, 1 (dangerous); deficiency of water, 1.

Of some of the defects enumerated above, a few particulars may be given:—

Furnace plates out of shape.—One of these cases occurred to an internally-fired boiler, simply from the want of strengthening hoops, and another to a boiler externally-fired, and of the class termed French or Elephant, having two horizontal tubes of 2 feet diameter, completely filled with water, and set immediately over the fire, and one upper cylinder of 4 feet 6 inches diameter, partly filled with water and partly filled with steam, connected to the two lower ones. One of these lower tubes gave way over the fire, the plate bulging downwards into a cup-shape, and the metal, which was originally three-eighths of an inch in thickness, being wasted away to three-sixteenths. The feed water was very sedimentary, and in this construction of boiler, as indeed in all those fired underneath, the sediment is apt to lodge in the most dangerous part, viz.—immediately over the fire. Instances of the danger attending the plan of external firing are constantly occurring.

It may be added that the plates of the lower cylinders in these Elephant boilers frequently become burnt away, in consequence of the steam being confined in the lower chamber, through imperfect circulation of the water. Also these boilers are extravagant in their consumption of fuel, and very dependent on the integrity of their brick-work setting, which is found to be a frequent source of trouble and expense; while the arrangement of fittings, such as water-gauges, &c., is complicated and inconvenient.

Fracture.—One of these cases occurred to an internally-fired double-story boiler, somewhat similar in construction to those of the Elephant class, with the exception that it had but one cylinder or trunk beneath the main chamber instead of two, while that was internally instead of externally-fired. The fracture took place at a transverse seam of rivets at the crown of the furnace immediately over the fire, the plates being buckled out of shape, and the rivets shorn through. It would appear that this injury arose from two causes. First:—Compression due to the expansion of the plates, consequent on their over-heating from the imperfect circulation of the water, to which these double-story boilers are always liable. Second:—Contraction, consequent on the injudicious practice of cooling the boilers too rapidly for cleaning, by the introduction of cold water. This plan of suddenly cooling

hot boilers with cold water cannot be too carefully avoided. Few practices convert a new boiler into an old one more rapidly.

A second case occurred to a boiler only recently placed under the charge of the Association, which was of the ordinary "Lancashire" construction, internally-fired, and set upon a mid-feather, and had failed at the transverse seams at the bottom of the shell, the fracture being what is commonly termed "seam-rending." In consequence of this, the boiler, although a new one, had been frequently repaired, in addition to being re-bottomed, and upwards of £200 expended upon it in little more than a year. These repairs, however, were ineffectual, the "seam-rending" recurring immediately on the boiler being set to work. This was due to the injudicious manner in which the feed-water was introduced, viz: at the opening for the blow-out at the bottom of the boiler. The simple heating of the feed, or its introduction near the surface of the water, through an internal perforated pipe, so as to disperse it and prevent its lying in a dead mass along the bottom of the boiler, would have prevented the injury.

Grooving: Internal.—Two cases of serious grooving or channeling have been met with at the flat crown plates of steam domes, consequent on their frequent buckling. A source, frequently of inconvenience, and sometimes of danger, would be avoided by dispensing with steam domes altogether.

Grooving: External.—Six cases have been met with during the past month which occurred at the bottom of multitubular boilers, from the leakage of the transverse seams. This leakage was due to the unequal contraction of the plates, produced by the imperfect circulation of the water in these boilers. Some of these channels were eaten nearly through the plate.

Cases of Deficiency of Water.—One met with during the last month was due to the watchman's keeping up the fires within the boiler without noticing that the water was out of sight in the gauge-glass. It appears that the boiler lost its water through leakage at the back pressure-valve. Owing, however, to the inlet being nearly as high as the furnace crowns, the damage done to them was but trifling. Where the feed is introduced a little above the level of the furnace crowns, they cannot be laid bare by the water either being drained or syphoned out.

Another case of deficiency of water, which occurred a short time since, arose from the failure of the sough, into which the waste pipe for the blow-out at the bottom of the boiler discharged. On the attendant's opening the blow-out, the brick-work blew up, and he was unable to get within reach to close the tap, so that the furnace crowns were laid bare and became overheated. In consequence, however, of the boiler being fitted with a low-water safety-valve which let off the pressure of the steam on the water's falling below the proper level the injury to the furnace crowns was very trifling. Had blowing-out from the surface of the water been adopted instead of from the bottom, the furnace crowns could not have been laid bare.

These two cases of injury afford an illustration of the advantage of the plan recommended by the Association of introducing the feed, as well as blowing-out, at the surface of the water, and it is thought that it would be well were this arrangement generally adopted.

Explosions.—No. 5 Explosion—the consideration of which was deferred from the last to the present report—was of a very serious character, resulting in the death of 13 persons, and in injury, in some cases, very severe to 10 others. It took place at an iron works, the boilers of which were not under the charge of this Association.

The explosion was compound, two boilers exploding together. These compound explosions, though remarkable, are not singular, and particulars of two have already been given in previous monthly reports.

In the present instance the series consisted of seven, in communication one with the other, and extended from west to east, the boilers themselves lying north and south, having their furnaces at the south end, and the chimney at the north. At the west end of the series stood the engine-house, close to which No. 1 boiler lay, and it was No. 1 and No. 2 that exploded. All the boilers were externally-fired, No. 6 and No. 7 being of plain cylindrical egg-ended construction, and the remainder including the exploded ones, being cylindrical in the shell, with one flue tube, and having rounded ends, not truly hemispherical, but such as are known in the district by the term of "bull-dog." The boilers were set upon mid-feathers, and fired underneath, the flames passing in the first instance along the left-hand side of the wall, then returning on the other, and finally passing through the internal flue tube to the chimney.

The rents in both the exploded boilers had taken place at the furnace or south end, and a portion of the shell containing three or four rings of plates had severed itself in each instance from the remainder. The main portion of the shells had flown northward, that of No. 2 to a distance of 80 yards, and that of No. 1 to 30 yards. The former had fallen beyond the works, but the latter immediately upon one of the puddling furnaces, knocking down the chimney, and demolishing the roof of the building. A fragment of the firing end of No. 1 had been thrown in a north-easterly direction to a distance of 130 yards. The engine-house, a massive stone building, three stories high, adjoining No. 1 boiler was unroofed, the whole of one side thrown down, and the remaining walls bulged and shaken. The bed of the boilers was completely torn up, and bricks and pieces of stone scattered in every direction. From these missiles many of the deaths occurred. One at a distance of about 105 yards, another at 80 yards, a third at seventy yards, and a fourth at 30 yards; while one person, though fortunately not killed, was struck and seriously hurt when standing 100 yards off; and I observed an edge stone, which, though it had fallen at a distance of 200 yards from the boilers, had yet cut its way into the ground with sufficient force to have killed any one it had struck. Those injured by the flight of the boilers themselves were comparatively few; while as many as six men were killed in the engine-house, by the steam and hot-water that played in upon them,

from the boilers remaining at work, after the explosion of Nos. 1 and 2, although the intervening steam pipe for a length of about 40 feet had been carried away.

Destructive and complicated as the results of the explosion were, the cause is very simple, as indeed it is in every case.

It appears that the water with which the boilers were fed was highly corrosive, a portion of it being derived from some coal workings a short distance up the valley in which the iron works were situated. This water had eaten into the surface of the plate in a most remarkable manner, and I have never seen any so severely affected. A considerable portion of the inside of the boilers, presented an appearance very similar to a honey-comb, the cells running closely one into another, and in many cases penetrating so deeply into the body of the plate as only to leave a mere film of metal about one-sixteenth of an inch thick. The corrosive action was specially active at the overlaps, where it eat away a deep channel, completely undermining the edge of the plate, and in some cases penetrating as far as the body of the rivet. This channeling was quite independent of mechanical action, being as severe at the transverse as at the longitudinal seams. The plates which were most affected by this corrosive action were those nearly over the fire-bridge at the bottom of the boiler, and in the vicinity of the feed-inlet. Those on the left-hand side of the midfeather wall, were more severely attacked than those on the right, in consequence, as it would appear, of the higher temperature, since the flames from the furnace passed on that side in the first instance. The intensity of the action diminished towards the back end of the boilers, and its effect upon No. 1 did not appear to be precisely similar to that on No. 2, the latter being deeply honey-combed, and the former eaten away more evenly. In No. 2 there could not have been less than 50 square feet of honey-combed surface.

With regard to the compound nature of the explosion: That the boilers did not burst simultaneously, but that the explosion of one caused that of the other, is attested by the fact that those at the various works at the time heard two distinct reports. The primary rent, it appears, was at a longitudinal channel immediately over the fire at the bottom of No. 1, the edge of the plate at this fracture for a length of some feet being reduced to a knife-edge This rent extended longitudinally throughout the first two or three rings of plate, and then assumed at the adjoining ring seam of rivets, a transverse direction running completely round the circle.

For the present month, 3 explosions have to be reported, from which 15 lives have been lost, and also 25 persons injured. Not one of the boilers was under the charge of this Association. The scene of the explosion has been personally visited in each case, and the cause investigated.

No. 6 Explosion was of a very fatal character, twelve persons being killed, and nineteen others injured. It occurred at an iron works, the boilers of which were not under the inspection of this Association. The exploded boiler was of the vertical furnace class, and heated by

the flames passing off from three iron furnaces. These flames played in the first instance on the outside of the boiler, then passed through three neck-openings into a central internal descending flue, and thence by means of a culvert to the chimney.

No. 7 Explosion, which resulted in the death of one person and in injury to six others, occurred at a saw-mill, and was due to the collapse of a furnace flue, and is an illustration of the necessity of strengthening all such flues with hoops or other suitable means.

The boiler which was not under the inspection of this Association, was of Cornish construction, 20 ft. in length, and about 4 ft. 6 ins. diameter in the shell. The furnace tube was slightly oval, very likely unintentionally so, through imperfect workmanship, and measured at the furnace mouth 2 ft. 6 ins. vertically, and 2 ft. 4½ ins. horizontally. The thickness of the plates in the shell was three-eighths, and in the furnace tube five-sixteenths of an inch.

The furnace tube had collapsed from the fire-bridge to the end of the boiler. These tubes usually collapse vertically the crown coming down, but in this case the movement had been horizontal, the two sides coming together. The fire-bridge had arrested the collapse at that part, and the crown-plates at the furnace end, which showed no signs of having been overheated, remained uninjured. The tube had severed at one of the ring seams of rivets midway between the fire-bridge and the end of the boiler, and the back end plate had been torn away from the shell; while the main portion of the boiler was thrown forwards across the stoke-hole, and the fragments blown in the opposite direction.

The damage done to the surrounding property was considerable, not so much perhaps from the peculiar violence of the explosion, as from the fact of the boiler being surrounded by buildings, many of which were dwellings. The works in which the explosion took place were leveled to the ground and reduced to a heap of ruins, while the surrounding houses were so damaged that they presented the appearance of having been bombarded, and some of them it is reported, had to be condemned on the following day, by the district surveyor, on account of their dangerous condition.

No. 8 Explosion, which resulted in the loss of two lives, afford an additional illustration of the danger so often pointed out in these reports, as attendant upon the use of externally-fired boilers, especially when they are fed with sedimentary water and not provided with efficient blow-out apparatus.

The explosion occurred at an iron works, the boilers of which were not under the inspection of this Association. The boiler in question was No. 3 in a series of eight, working side by side, and connected together both by the steam pipe and feed pipe. They were of plain cylindrical egg-ended construction, externally-fired, and set with a flash flue: their length being 40 ft., their diameter 5 ft., and thickness of plate three-eighths of an inch.

The boiler gave way at one of the transverse seams of rivets situated about 8 ft. from the front end, and very near to the fire-bridge.

The rent commenced at the bottom, and then continued throughout the entire circle of rivets, severing the shell completely into two sections, which flew in a straight line in opposite directions. Judging from objects struck in its flight, the furnace end of the shell must have assumed a considerable angle of elevation immediately on quitting its seat. Two boilers on each side of the exploded one were dislodged from their original position and thrust laterally. Had not the flue below them been unusually roomy, and thus afforded considerable vent for the steam and water which escaped from the rent, it is more than probable, that the explosion would have spread to the sister boilers as it has done in other cases, and perhaps have involved the whole series.

This explosion was not due to shortness of water, as an examination of the condition of the plates clearly showed, neither was it due to the excessive pressure.

Having now given the principal facts of the preceding explosions, a few general remarks, in conclusion, may not be out of place.

It appears that, during a period of scarcely three months,—*i.e.*, from the commencement of the present year, up to March 25th, the day to which report is made up,—there have occurred eight explosions, from which 29 persons have been killed, and 42 others injured; while five persons connected with the management of these boilers have been committed for manslaughter.

It cannot have escaped attention how extremely simple the cause of all these explosions have been; in three cases, the choking of the feed pipes through frost; in two others, wasting of the plates to the thickness of a sheet of paper; and in the sixth, the mal-construction of the boiler.

Iron works appear to maintain their position at the head of the list, both for the number and fatality of their explosions, and, under these circumstances, it may not be unwise to re-consider the policy now generally adopted at these works, viz: that of employing the more primitive description of engineering arrangements, in preference to those of modern character, and now widely adopted in other branches of industry. It is often argued that the rough class of labor obtainable at iron works necessitate the continuance of primitive and rough mechanical appliances; an argument, it is thought, that admits of question. True economy in engineering matters is only to be found in the employment of the best material and workmanship. The continued use of inferior and rough boilers, tends to perpetuate inferior and rough workmen; while on the other hand, the use of a superior description, would stimulate and raise them; and should the statement which is sometimes made be correct, that iron works will not supply workmen of the requisite stamp, nothing could be easier than to import them from other departments of engineering. Where boilers of a superior class have been adopted, they have been found to be productive of economy in working, as well as of human life, and there can be no reason to doubt that these advantages would follow their adoption at iron works.

On the Supposed Nature of Air prior to the Discovery of Oxygen.

By George F. Rodwell, F.C.S.

From the London Chemical News, Nos. 214 and 215.

(Continued from page 176.)

4. *Invention of the Air Pump.*—About ten years after the discovery of the pressure of the air, Otto Von Guericke,* a burgomaster of Magdeburgh, made a number of experiments, with a view of obtaining a larger vacuous space than that which could be procured by Torricelli's method; for, although the behavior of certain bodies in a vacuum had been tried by introducing them into the Torricellian vacuum, this mode of experimenting was obviously attended with numerous difficulties, and but few substances could be placed in a vacuum obtained by such a process.

Guericke conceived that he could effect his object by completely filling a vessel with some substance which could be removed without allowing air to take its place. He, accordingly, filled a wooden cask with water, and fitted into it a syringe, all other parts being closed as securely as possible; he then pumped out the water, expecting that a vacuum would be produced in the cask, but air readily entered, by the crevices, to occupy the space which the water left.†

Having previously observed, however, that air could be made to expand to any extent, he determined to cause air to leave a vessel, in virtue of that property, and to prevent its return by external means. In order to effect this, he took a copper globe, and fitted into it a syringe; when the pumping was commenced, the piston worked somewhat easily, but became more and more difficult to raise, and in a few minutes the globe was compressed with a loud noise.

Guericke next procured a glass globe, furnished with a straight brass tube, containing a stop-cock, by means of which, free communication with the interior of the globe could be established or prevented; to the lower part of the tube, and inclined to it at an angle of 45°, a syringe, having a solid piston, was adapted. Near the lower end of the syringe, a lateral valve opening outwards was placed, and at the juncture of the syringe with the tube there was a second valve, opening into the syringe. The valve and stop-cock were immersed in water to prevent air from leaking into the apparatus.‡ It is obvious that, on raising the piston of such an air-pump, the air in the globe will expand, open the lower valve, and escape into the body of the syringe, while the lateral valve will be kept closed by the pressure of the air; and, on depressing the piston, the lower valve will close, and the lateral valve will open to allow the air in the body of the syringe to escape. A good vacuum could be obtained by this instrument, but it required a great deal of pumping to produce it.

* Born, 1602. Died, 1686.

† See "Ottonis de Guericke Experimenta Nova (ut Vocantur) Magdeburgica de Vacuo Spatio." Amstelodami. 1672.

‡ See "Mechanica Hydraulico Pneumatica," by Gaspar Schottus. 1658. Also, "Technica Curiosa," (1664), by the same author.

Soon after the invention of the air-pump, Guericke constructed the "Magdeburgh hemispheres," a full account of which, Schottus informs us, was first given to him in July, 1656. In Guericke's "Experimenta Nova," there is a picture which conveys to the mind a good idea of the enormous pressure of the air. In the centre of this picture we observe a pair of large Magdeburgh hemispheres, to each of which eight horses are attached, and, although most evidently urged to their uttermost by men, who flourish aloft gigantic whips, they are unable to effect the division of the sphere.

Among other experiments made by Guericke with his air-pump, we may mention the following:

1. A sparrow placed in the receiver died on exhausting.
2. A candle was extinguished in an exhausted receiver, because, says Guericke, *fire consumes air.*
3. Water was found to rise to a great height in an exhausted tube, and when the exhausted receiver of the pump was opened under water, water violently entered, and almost filled it.
4. A vessel exhausted of air was found to be considerably lighter than before exhaustion.
5. A bell made to ring in an exhausted receiver was inaudible until air had been admitted.

Kircher* had previously tried this experiment in the Torricellian vacuum by ringing a bell, the clapper of which was of iron, and was moved by the alternate approximation and withdrawal of a magnet. Kircher states that the sound was distinctly audible, from which we may infer that he suspended his bell within the tube by means of some substance capable of transmitting the vibrations to the tube.

Guericke did not believe the air to be elemental, and considered it incapable of being converted into water.

The following is his definition of air.

"Aer est nihil aliud quam expiratio aut effluvium, aquarum, terrarumque, et aliarum rerum corporearum."

The science of Pneumatics, which took its rise immediately after the invention of the air-pump, was greatly extended in England by some of that band of experimental philosophers which arose shortly after the death of Bacon. Foremost in that band, if not the very leader of it, was Robert Boyle,† a man who, despising all previous philosophies, applied himself vigorously to carry out the ideas of Lord Bacon. Boyle had the greatest reverence for the "Novum Organum," and there could not be a mind better adapted than his for putting in practice the "new philosophy." He was the very man that was wanted, at a time when speculative philosophy, unaided by experiment, was being changed for experimental philosophy, aided by inductive reasoning.

"But our hope of further progress in the sciences," writes Bacon, in the "Novum Organum,"‡ "will then only be well-founded when numerous experiments shall be received and collected into natural

* Born, 1601. Died, 1680.

† Born, 1626. Died, 1691.

‡ Book 1, Aph. 99.

history, which, though of no use in themselves, assist materially in the discovery of causes and axioms, which experiments we have termed, enlightening, to distinguish them from those which are profitable. They possess this wonderful property and nature, that they never deceive or fail you; for being used to discover the natural cause of some object, whatever be the result, they equally satisfy your aim by deciding the question."

Now, Boyle was the man to thus promote the welfare of the sciences; he was not one to cultivate them for profit to himself, or for renown, but he chose rather to study science for the pure love of the thing; and such men only have a right to bear the title of Philosopher, and by them alone is science permanently benefited. Bacon was the architect who planned out a glorious structure, and Boyle was one of the master-builders of his day; and he laid the foundation of a part of that building so securely, that it still endures; and although now and then we have to remove a brick which has fallen to decay, we still continue to build on that foundation, and the workmen increase, and the building rises rapidly,—let us hope it may be soon roofed in.

The writings of Boyle extend over a period of thirty years; they are excessively prolix, but with so few previous experiments on most of the subjects, it was difficult to avoid describing the most trivial effects: we must remember, moreover that he wrote of phenomena hitherto almost unknown to the human mind, and we must overlook the tediousness of the accounts of his experiments, which, although they appear very detail to us, were probably not too much so for his contemporaries.

Boyle's first work on the air appeared in 1661, and was entitled "New Experiments, Physico-Mechanical, Touching the Spring of the Air and its Effects; made for the most part in a new Pneumatical Engine." The entire work is written in the form of a letter to Lord Dungarvan, and is dated December 20, 1659.

So soon as Boyle heard of the invention of Otto Von Guericke's air-pump, he determined to construct one which should be less cumbrous, and should not require to be placed in a vessel of water. He mentioned his wish to his friends, Hooke and Greatorex, and, after several unsuccessful trials, Hooke made for him an air-pump which was considered to be superior to Guericke's. It consisted of a hollow vertical cylinder of cast brass, fourteen inches long and three inches internal diameter, the lower end of which was open, and the upper closed, with the exception of a small orifice in the line of the axis of the cylinder, and a tapering, lateral orifice, into which a brass stopper, serving as a valve, fitted air-tight. A solid piston, to which vertical motion was given by means of a rack and pinion, worked within the cylinder. The receiver* was a glass globe capable of containing thirty wine quarts, fitted with a stop-cock, the shank of which was cemented

* No one can have failed to observe how singularly inapplicable the name of "*receiver*" is to that part of an air-pump which it is desired to *empty* as completely as possible; the name was given by Boyle, who, in speaking of the vessel to be exhausted, says, "which we, with the glass-men, shall often call a receiver, for its affinity to the large vessels of that name, used by chemists."

into the upper orifice of the cylinder; and in that part of the globe opposite the stop-cock there was a circular-lipped opening four inches in diameter, to which a brass ring (diminishing the diameter of the opening to three inches) was cemented; the ring was closed by an "exquisitely-ground" brass plate, containing a half-inch circular orifice closed by a brass stopper, which could be turned, when the receiver was exhausted, without admitting air. The whole apparatus was arranged vertically, so that it was terminated above by the opening of the receiver for the introduction of bodies to be experimented upon, and below by the open orifice of the brass cylinder.

Before commencing to exhaust the receiver, the piston and all the joints were well oiled, the piston pushed to the top of the cylinder, and the stop-cock and lateral valve closed; the piston was then drawn to the bottom of the cylinder, and the stop-cock opened to allow air to rush from the receiver into the vacuous space; the stop-cock was now closed, the valve opened to allow of the exit of the air in the body of the cylinder, and the piston again forced to the top of the cylinder; the same process was repeated until the receiver was exhausted.

I do not consider this first air-pump of Boyle's by any means so ingenious a contrivance as that of Otto Von Guericke. The arrangement of the apparatus was indeed different, but the method of exhaustion was the same, more clumsily effected; the stop-cock of Boyle's pump played the same part as the lower valve of Guericke's, and the lateral valve of the former the same as the lateral valve of the latter, with this disadvantage, however, that the valves of Boyle's pump had to be worked by hand, while those of Guericke's were worked by the air.

Some of the more important of the forty-three experiments detailed in the treatise we are considering, are given below:

In the first experiment Boyle explains by what means the receiver of his air-pump is exhausted, and he affirms that most of the experiments which he is about to relate may be explained by bearing in mind the fact, "that there is a spring or elastical power in the air we live in."

He compares the air to a fleece of wool, or a dry sponge,* or says we may admit Des Cartes' theory, that the air is made up of innumerable flexible particles, which are so moved about by the motion of the celestial matter in which they swim, that each particle keeps those around it at a distance. Boyle prefers to believe that the air itself is elastic, rather than to admit Des Cartes' theory.

The height of the atmosphere, according to Kepler, is only eight miles, but Ricciolo makes it probable that it extends to a height of fifty miles. "Can we not, therefore, well imagine," writes Boyle, "that

* Hero, of Alexandria, who was acquainted with the elasticity of the air, compares it to horn shavings, or a sponge.

Pascal, as we have seen in the previous paper, compared the air to wool, a favorite simile of Boyle's throughout this work, but whether taken from Pascal or not it is difficult to say. Pascal's treaties "On the Weight of the Mass of Air." was not published till 1663, but Boyle may have heard of the comparison from some one who had conversed with Pascal.

the weight of a column of air of such a height will keep 'the little springs' of the air bent, and that when that weight is removed they will straighten themselves.

Experiment 2. When a stoppered receiver was exhausted of air, great force was found to be necessary to remove the stopper.

From the length at which this experiment is detailed it would appear that Boyle had not heard of the Magdeburgh hemispheres, although they were invented several years previously.

Experiment 4. A half-blown bladder placed in the receiver became fully expanded on exhausting.

This experiment had been previously tried by Roberval, by introducing a nearly empty carp's bladder into the Torricellian vacuum.

Experiment 5. When the exhaustion was long-continued the bladder burst; a partially-filled bladder also burst when held near a fire.

Experiment 6. In order to determine to what extent a known quantity of air could be made to expand, when pressure was removed from it, Boyle took a phial containing four or five drachms, tied an empty lamb's bladder over its mouth, and placed it in the air-pump receiver; exhaustion was continued until the bladder was fully expanded, and by comparing the capacities of the phial and bladder, the air was found to have expanded to nine times its original bulk.

This mode of experimenting was evidently borrowed from Bacon, who, in the 40th Aph. of Book 2 of the "Novum Organum," describes an experiment which he made in order to determine the space occupied by the vapor produced from a liquid occupying a known space; in other words, the ratio between the spaces occupied by a substance when in the liquid and gaseous states. This experiment, although it could only give most fallacious results, is well worthy of record, inasmuch as it was made at a time when experiments of any kind were rare, and quantitative experiments most rare.

Bacon filled an ounce phial, of known weight, with the lightest liquid with which he was acquainted (spirits of wine), weighed, to determine the amount of spirit taken, and tied an empty bladder, containing about two pints when full, over the mouth of the phial; the spirit was heated until the bladder was fully expanded, when the heating was discontinued, the bladder pricked to allow the vapor to escape, and then removed from the phial; on re-weighing the phial, the amount of spirit converted into vapor was found, and the space it occupied being compared with the contents of the bladder, it was shown that the spirit of wine occupied in the state of vapor 100 times the bulk it possessed in the liquid form.

But we must return to Boyle's experiment on the expansion of air under diminished pressure. After making the experiment with the bladder and phial, he devised a far more accurate method, which was to place within the receiver a tube, closed at one end, to which a divided scale was attached, and the capacity of which was known. The tube was entirely filled with water, with the exception of the space occupied by a small quantity of air of known bulk, and its lower end was placed in a vessel of water. When pressure was removed,

the air within the tube expanded and depressed the column of water, and the amount of expansion was shown by the scale. By this means it was found that air expanded to thirty-one times its bulk, but the tube was too short, and the air still continued to expand, when the experiment had to be stopped. He next made use of a tube three feet long, passed it air-tight through the cover of the receiver, and placed the phial of water, into which its open orifice dipped, within the receiver; a known quantity of air was now found to expand to 152 times its previous bulk; it is obvious, however, that the inaccuracy of the experiment increases with the length of the tube, because, when the exhaustion has proceeded to a certain extent, the column of water will fall, not only on account of the expansion of the air above it, but also because its own weight can partially overcome the pressure of the rarefied air in the receiver.

Experiment 7. A small globe of very thin glass filled with air, was placed in air-tight connexion with an exhausted receiver: on suddenly turning the stop-cock, so as to open free communication with the receiver, the globe was not broken, probably, Boyle says, on account of its shape.

Experiment 8. The helmet of a glass alembic was fitted with a stop-cock, every other part being closed; the shank of the stop-cock was cemented into the upper orifice of the pump-barrel; on exhausting, the helmet was broken.

Experiment 9. A glass tube, open at both ends, was cemented firmly into the neck of a glass phial containing a small quantity of water, so that its lower end reached nearly to the bottom of the phial; the latter was placed in a small receiver, and the tube passed air-tight through the cover; on exhausting, the phial was burst,* because, says Boyle, the inside of the phial had to bear the whole weight of the atmosphere, and there was no corresponding pressure to counterbalance it on the outside. The thinnest glass vessel is not broken in the air, because both the inside and outside are equally pressed; in other words, it is submitted to no pressure at all.

Experiment 10. A lighted candle was placed in the receiver; on exhausting, it was extinguished. When burnt in the closed receiver, without exhausting, it continued alight for a much longer time.

Experiment 11. A wire-basket, filled with ignited charcoal, was suspended in the receiver; on exhausting, the charcoal ceased to glow

* The phial burst with such violence that it cracked the air-pump receiver,—a not unfrequent occurrence, however, from other causes; and, as receivers were difficult to procure, Boyle was in the habit of covering the cracks with cement, and afterwards with diachylon plaster. The following is his account of the preparation of the cement:—"The plaster was made of good quicklime, finely poudred, and nimbly ground with a pestle in a mortar, with a quantity (I know not how much precisely, not having the essays in this place) of scrapings of cheese and a little fair water, no more than is just necessary to bring the mixture to a somewhat soft paste, which, when the ingredients are exquisitely incorporated, will have a strong and stinecking smell. Then it must be immediately spread upon a linnen cloath of three or four fingers' breadth, and presently apply'd, lest it begin to harden. But if your Lordship had seen how we mended with it receivers, even for the most subtle Chymicall spirits, you would scarce wonder at the service it hath done in our Pneumatical Glass."

sooner than when the receiver was not exhausted; but a piece of red-hot iron did not appear to lose its redness sooner in a vacuum than when placed in the closed receiver.

Experiment 12. A piece of lighted match, "such as souldiers use," was placed in the receiver; it was quickly extinguished, and an immense quantity of smoke was produced.

Experiment 13. In order to see whether the match was extinguished for want of air, or because the smoke pressed upon it, and stifled the flame, a partially-blown bladder was introduced with the match, to see if the fumes exerted appreciable pressure, but the bladder expanded quite as readily as when it was placed in the receiver without the match.

Experiment 14. Gunpowder could be fired in an exhausted receiver, and a flint and steel meeting together in collision produced sparks.

(To be Continued.)

On the Spectra of some of the Fixed Stars.

From the London Chemical News, No. 236.

A paper "on the spectra of some of the fixed stars," by W. Huggins, F.R.A.S., and Professor W. A. Miller, was then read. After a few introductory remarks, the authors describe the apparatus which they employ, and their general method of observing the spectra of the fixed stars and planets. The spectroscope contrived for these inquiries was attached to the eye-end of a refracting telescope of 10 feet focal length, with an 8-inch achromatic object-glass, the whole mounted equatorially and carried by a clock-movement. In the construction of the spectroscope, a plano-convex cylindrical lens, of 14 inches focal length, was employed to convert the image of the star into a narrow line of light, which was made to fall upon a very fine slit, behind which was placed an achromatic collimating lens. The dispersing portion of the arrangement consisted of two dense flint-glass prisms, and the spectrum was viewed through a small achromatic telescope with a magnifying power of between 5 and 6 diameters. Angular measures of the different parts of the spectrum were obtained by means of a micrometric screw, by which the position of the small telescope was regulated. A reflecting prism was placed over one half of the slit of the spectroscope, and by means of a mirror, suitably adjusted, the spectra of comparison were viewed simultaneously with the stellar spectra. This light was usually obtained from the induction spark taken between electrodes of different metals. The dispersive power of the apparatus was sufficient to enable the observer to see the line *Ni* of Kirchhoff between the two solar lines D; and the three constituents of the magnesium group at *b* are divided still more evidently.* Minute details of the methods adopted for testing the exact coincidence of the corresponding metallic lines with those of the solar and lunar spectrum

* Each unit of the scale adopted was about equal to $\frac{1}{1800}$th of the distance between A and H in the solar spectrum. The measures on different occasions of the same line rarely differed by one of these units, and were often identical.

are given, and the authors then proceed to give the results of their observations. Careful examination of the spectrum of the light obtained from various points of the moon's surface failed to show any lines resembling those due to the earth's atmosphere. The planets Venus, Mars, Jupiter, and Saturn were also examined for atmospheric lines, but none such could be discovered, though the characteristic aspect of the solar spectrum was recognised in each case; and several of the principal lines were measured, and found to be exactly coincident with the solar lines. Between forty and fifty of the fixed stars have been more or less completely examined; and tables of the measures of about 90 lines in Aldebaran, nearly 80 in α Orionis, and 15 in β Pegasi are given, with diagrams of the lines in the two stars first named. These diagrams include the results of the comparison of the spectra of various terrestrial elements with those of the star. In the spectrum of Aldebaran coincidence with nine of the elementary bodies were observed, viz: sodium, magnesium, hydrogen, calcium, iron, bismuth, tellurium, antimony, and mercury; in seven other cases no coincidence was found to occur. In the spectrum of α Orionis five cases of coincidence were found—viz: sodium, magnesium, calcium, iron, and bismuth; whilst in the case of ten other metals no coincidence with the lines of this stellar spectrum was found. β Pegasi furnished a spectrum closely resembling that of α Orionis in appearance, but much weaker; only a few of the lines admitted of accurate measurement, for want of light; but the coincidence of sodium and magnesium was ascertained; that of barium, iron, and manganese was doubtful. Four other elements were found not to be coincident. In particular, it was noticed that the lines C and F, correspond to hydrogen, which are present in nearly all the stars, are wanting in α Orionis and β Pegasi. The investigation of the stars which follow is less complete, and no details of measurement are given, though several points of much interest have been ascertained. Sirius gave spectrum containing five strong lines, and numerous finer lines. The occurrence of sodium, magnesium, hydrogen and probably of iron, was shown by coincidence of certain lines in the spectra of these metals with those in the star. In α Lyræ the occurrence of sodium, magnesium, and hydrogen was also shown by the same means. In Capella sodium was shown, and about twenty of the lines in the star were measured. In Arcturus the authors have measured about thirty lines, and have observed the coincidence of the sodium line with a double line in the star spectrum. In Pollux they obtained evidence of the presence of sodium, magnesium, and probably of iron. The presence of sodium was also indicated in Procyon and α Cygni. In no single instance have the authors ever observed a star spectrum in which lines were not discernible, if the light were sufficiently intense and the atmosphere favorable. Rigel, for instance, which some authors state to be free from lines, is filled with a multitude of fine lines. Photographs of the spectra of Sirius and Capella were taken upon collodion; but, though tolerably sharp, the apparatus employed was not sufficiently perfect to afford any indication of lines in the photograph. In the concluding portion of their paper, the authors apply the facts observed

to an explanation of the colors of the stars. They consider that the difference of color is to be sought in the difference of the constitution of the investing stellar atmospheres, which act by absorbing particular portions of the light emitted by the incandescent solid or liquid photosphere, the light of which in each case they suppose to be the same in quality originally, as it seems to be independent of the chemical nature of its constituents, so far as observation of the various solid and liquid elementary bodies, when rendered incandescent by terrestrial means, appears to indicate.

Proceedings of the Royal Society, May 26, 1864.

For the Journal of the Franklin Institute.

On the Parabolic Construction of Ships. By JOHN W. NYSTROM.

The following communication has been received by me.

TRIEST, Austria, June 8th, 1864.

GENTLEMAN:—After having perused your treatise on the Parabolic Construction of Ships, your Pocket Book of Mechanics, 9th edition, and a Lecture on the Parabolic Construction, published in the *Journal of the Franklin Institute*, re-published in the *Artizan* for July, 1864, in London, I take the liberty of sending you the theories that sustain Chapman's hypothesis, in which you have not succeeded; with the hope that you will correct your theory, based on wrong hypothesis, before going forward with your *treatise*, now in progress, and that it really will be brought to perfection; and should these proofs be accepted, I may avail myself of another opportunity to forward some more extensive details on the Parabolic Construction, communicated to my family in the year 1806, from Chapman himself, (sending a work entitled, "Försök till en Theoretisk Afhandling att gifva ett Linie skepp dess râtta storlek ach form. Likaledes för Fregatter och mindre Bevârade Fartyg af F. H. af Chapman Carlscrona, 1806,") which was the result of labors of the latter years of Chapman's life, that he dedicated it, as his *testament* or memorial of what he had been able to contribute to it, to all enlightened *admirers* of this noble science.

In case you would be so very kind as to answer this letter, I beg you to have the kindness, if you do not publish the theoretic part of the Parabolic System in your progressional work, to make me acquainted with it, in order that I may be able to know if I have something still to learn of it.

Very respectfully,

ANTHONY PANFILLI,
No. 990, Triest, Austria.

Mr. JOHN W. NYSTROM, C. E. [COAT OF ARMS.]

I have received half a dozen letters of this kind from ship-builders in Europe, which all seem to agree that I am wrong in my Parabolic Construction; but they do not state why or wherein I am wrong, only refer me to Chapman's formulas. I have answered several commu-

nications, and requested them to explain why or wherein I am wrong; and if any light is obtained on the subject, I shall not fail to communicate the same to the Franklin Institute. In the meantime, I shall go on, uninterrupted, with my work on the Parabolic Construction, as I am under the impression that they only suppose I am wrong merely because my hypotheses differ from those of Chapman. I may consider myself a pupil of Chapman's school, because it is on his Parabolic method I have started, and progressed as explained in the *Journal of the Franklin Institute* and in the Lecture at the Polytechnic College, Philadelphia. The first part of that Lecture, which explains the inefficiency of Chapman's hypotheses, has not yet been published, but the following is an extract of the same:

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Extracts from the first part of the Lecture on the Parabolic Construction, at the Polytechnic College, Philadelphia, February 11, 1864.

It is proposed to explain to you, this evening, a new method of constructing ships, called the *Parabolic Construction*. It is so called because it is based on the formula for a parabola, whereby all the lines in a ship can be constructed.

The Parabolic System of constructing ships was originated by the celebrated Swedish naval architect, Chapman, nearly a century ago; at which period it was well received among ship-builders, but on account of its then incomplete form, restricting constructors to particular shapes, it was gradually abrogated, until no trace could be found of it, even in works on ship-building. Mr. Chapman hit upon the fortunate idea that the cross-sections of the displacement of a vessel ought to follow a certain progression, in order to present the least possible resistance when moving through the water. He collected a great many drawings of ships of known good and bad performances, and made the following investigation. On each drawing he transformed the cross-sections of the displacement into rectangles of the same breadth as the greatest beam of the load-water-line of the vessel; placed their upper edges in the plan of the load-water-line, by which he found that the under edges of the rectangles formed a bottom, the curve of which were parabolas in ships of known good performances.

Let the accompanying figure, 1, Plate IV, represent a ship with the load-water-line, w, dead-flat cross-section *a* ℵ *b*, formed into the rectangle *a b c d*, and *i* ℊ *i* another cross-section formed into a rectangle *e f g h*, so that the breadth *e f* is equal to *a b*; then the line *k l m*, Fig. 1, Pl. IV, forming the bottom of the rectangles, should be a parabola with the vertex at *k*, and *k o* the axis of the abscissa.

Mr. Chapman found that the parabola so obtained did not terminate at the stem *n*, but fell a little short at *m*. The deviation *m n* was very small in vessels of his days, but in modern vessels it is more considerable, showing that there must be a point of inflection *p* in the curve. However erroneously we may set out in quest of an object, experience generally leads us towards correct scientific principles. In the case before us, experience has increased the deviation *m n*, and

we know that inasmuch as nature admits of no physical by-laws, the curve cannot be a plain parabola. It is this increasing deviation $m\ n$ which has led me to investigate the subject more carefully; starting on the principle that the resistance to a body in motion in a fluid, is a function of the square of the sine of the angle of incidence to the motion. Let $a\ b\ c\ d$, Fig. 2, Pl. IV, be a body in motion in a fluid, in the direction $a\ c$; then the resistance to that body is found by experiments to be nearly as the square of the sine of the angle v; the beam $b\ d$ being constant.

From this it appears that the proper progression of the cross-sections should be as the square of the ordinates in a parabola.

Let Fig. 3, Pl. IV, represent a vessel with the dead-flat $\mathfrak{X}$ and stem n. Draw the cross-section $a\ \mathfrak{X}\ b$ and the rectangle $a\ b\ c\ d$, as before described; draw a parabola $k\ l\ n$ of any desired order, terminating at the stem; then the proper progression of the cross-sections should be as the square of the ordinates β. Let $b = 1$, then the ordinates β will be fractions of b, and the square β^2 multiplied by the area of the dead-flat cross-section $\mathfrak{X}$, would give the proper area of the ordinate cross-section θ, or $\theta = \mathfrak{X}\ \beta^2$, Fig. 3, Pl. IV. The line $k\ m\ n$ should then indicate the proper progression of the ordinate to cross-sections θ. The areas $e\ f\ g\ h = i\ \theta\ i$.

Here the Lecture was continued on the properties and formulas for the Parabola, which is not necessary to repeat on this occasion. Arrived to the following formulas, referred to the rectangular co-ordinates half length and breadth of the vessel:

$$(a\ b) = b\left(1 - \frac{y^n}{l}\right) \quad . \quad . \quad 1$$

$$(a\ \theta) = \mathfrak{X}\left(1 - \frac{y^n}{l}\right)^2 \quad . \quad . \quad 2$$

The formula 1 gives the plain parabola $o\ \beta\ s$, Fig. 4, Pl. IV, and formula 2 the parabolic cyma $o\ \theta\ s$, of which the latter indicates the proper progress of the ordinate cross-section of the displacement of a vessel, and agrees with vessels constructed for speed of the present day, while old vessels come nearer to the plain parabola $o\ \beta\ s$, as constructed by Chapman.

I have investigated the progression of the cross-sections in a great many vessels, from most parts of the world, as will be seen in a treatise on the Parabolic Construction of Ships now in progress. Many American vessels agree perfectly with formula 2, of which the U. S. frigate *Niagara*, constructed by the late Mr. Steers, is one. The formula 1, which embodies Chapman's method, is therefore not applicable in modern ship-building, which I think, is the reason why the original parabolic system has not been more generally adopted. It is not always necessary to pay the greatest attention to speed, as there are many other circumstances of greater importance, namely, freight, shallow water, location of metacentre and centre of gravity of the vessel; for which it becomes necessary to arrange the parabolic construction of ships, so that it will accommodate itself to all the require-

ments, as well as to the taste of the ship-builder. This can be accomplished by raising the ordinate β to any arbitrary power, which we will designate with the letter q, and call it the power of the exponent n, when the final formula will appear:

$$\beta = b\left(1 - \frac{y^n}{l^n}\right)^q \quad . \quad . \quad 3$$

This is the general formula for the parabolic construction. The balance of the Lecture has appeared in the *Journal of the Franklin Institute.*

It does not appear that Chapman attempted to form the water-lines and frames of a vessel by the parabolic method. He says the area of the cross-sections can be approximated by a parabola, placing the vertex at the keel, which cannot give a proper shape to the frame. Inasmuch as the displacement of a vessel is the integral of the areas of the water-lines and cross-sections, and that those areas are integrals of the ordinates in the frames and water-lines, they are all convertible into one another by a common formula, which is the formula 3, and which formula embodies Chapman's system completely; simply by placing $q = 1$. But, by so doing, the constructor is restricted to a stiff and obstinate guide, which will not yield to his taste, and we have the result before us: namely, the ship-builder assumes his independence. It would be futile to attempt to introduce a system of constructing ships that would not accommodate itself to the taste of the constructor. By Chapman's system, when the length, breadth, depth, and the displacement are given, then, the sharpness of the vessel is obdurately fixed, while by an arbitrary value on q the sharpness and ease of the lines can vary considerably, and accommodate itself to the taste of the architect.

Suppose the area, length, and breadth of the load-water-line of a vessel are given, which is substantially the same as if the displacement, dead-flat, cross-section, and length were given; then Chapman's method will produce the fixed line, say *o m m s*, fig. 5, Pl. IV, while the formula 3 will produce any variety of lines, as *o o o s*, or *o e e s*, or if we wish to go to the extreme the wrong way, we can produce the line *o n n s;* in fact, the formula 3 can manipulate the displacement the same as one can work a lump of soft clay in his hands. What more is wanted? This is a property of my parabolic system which does not appear to have been appreciated by my correspondents on the subject; but when once fairly laid before them, I am assured they will be the first to appreciate and acknowledge its utility.

This formula 3 is not limited only to ship-building; but is applicable to a great variety of physical laws. It embraces all properties of heat, such as the pressure and temperature of steam, the law of dilatation, total and specific heat in matter. I prefer to use this formula for the rude and empirical mode of interpolation, and most invariably succeed in bringing it in.

Artificial Light and Lighting Materials. By B. H. Paul, Esq.

From the Journal of the Society of Arts, No. 593.

(Continued from page 210.)

Discussion.—Dr. Marcet, F.R.S., understood Mr. Paul as directing attention to the heating action produced by various lighting agents. This property, no doubt, varied very much in the different agents used —some gave out more heat than others, and some evolved more carbonic acid gas than others. Coal gas produced less carbonic acid than petroleum and other substances of that kind. This subject was important in connexion with the effect of artificial light upon health. No doubt in rooms where lights were burnt for a considerable time, unless proper regard were paid to ventilation, a large amount of noxious gases were evolved. These gases consisted of carbonic acid, sulphurous acid, and others of a more or less pernicious nature. In some cases in rooms where lights were burnt in which the combustion was not complete, there was a formation of carbonic oxide which was most poisonous. When explosions took place in mines not only carbonic acid was formed, but also carbonic oxide, and these were both destructive of life in a very short time, but each separately had a different action in the destruction of life. Carbonic acid gas destroyed life comparatively slowly, while carbonic oxide immediately struck down any one who breathed it. In the case of the lamentable explosion at the Hartley colliery, which was attended with so large a loss of life, he had no doubt that a mixture of carbonic acid and carbonic oxide was formed, and he hoped, for the sake of the victims themselves, that the latter predominated, inasmuch as their sufferings would be less protracted than under the influence of carbonic acid alone. The action of these gases produced such different appearances after death that it could be ascertained by which of them the death had been caused. Carbonic acid gas rendered the blood left in the veins of a dark color, while in cases of death from carbonic oxide the red color of the blood was heightened. The fact of these gases being generated by the combustion of the agents used for lighting purposes made it necessary that the utmost attention should be paid to ventilation. He thought the plan of lighting adopted in the room in which they were assembled was so perfect that it was impossible there could be any accumulation of noxious gases, while the light itself, by being concentrated into one large focus, was of a very efficient character, and ventilation was greatly promoted. The subject of lighting by petroleum having been alluded to by Mr. Paul, he could have wished that some further information had been given with regard to the Rangoon petroleum. From that material had been manufactured a very perfect lighting agent under the title of Belmontine, but he was afraid the supply of that petroleum, as indeed of many other kinds, was considerably on the decrease, but he had no doubt that other sources of supply would be obtained as the original ones failed.

Dr. Bachhoffner said on a recent occasion when they were favored with a paper on an analogous subject to this, he expressed a some-

what strong opinion with reference to the use of hydro-carbons as lighting agents, which opinion he saw no reason to retract. He still maintained that for the ordinary oils of commerce a sufficient guarantee should be given as to their non-inflammability at a temperature below a certain fixed standard; otherwise there was danger in their use for domestic purposes. He did not dispute the correctness of Mr. Paul's statement that, if these oils were used under proper precautions, they would be safe enough: but the great difficulty was to ensure those precautions in ordinary households. Most of the lamps at present in use required to be trimmed and lighted in a peculiar way, and with great care, and this could hardly be trusted to servants. He had no objection to offer against the use of these lighting agents, provided a satisfactory guarantee were given by the retailers of the article that it would not explode at a temperature below 100° at least, though he would rather have a margin of 20° or 30° above that. There was at present a certain amount of danger in the use of these hydro-carbon oils. He was aware that the same objections were raised against ordinary gas on its first introduction. He had no doubt they could easily blow up a house with gas if they were so disposed, but this would only result in most cases from great carelessness. The peculiar odor of gas was a great safeguard in its use, for any escape was at once detected. He remembered there was once an attempt made to deprive coal gas of its odor, but he regarded that as the greatest security they could have against accidents, for if they allowed the taps to be turned on they were apprized of the fact through the medium of the olfactory sense. With regard to petroleum, until there was a satisfactory guarantee when hydro-carbon oils were purchased, that they were not explosive at a temperature below something like 130°, he considered they were not safe for general domestic use.

Mr. Robinson, as one who had had some experience in the use of paraffin oil, fully confirmed the views expressed by the last speaker, as to the necessity for a sufficient guarantee being given as to the temperature at which these oils would inflame. Without that guarantee he considered their use objectionable and dangerous. The system under which they were at present sold was extremely unsatisfactory. A short time since he purchased some paraffin oil which was guaranteed not to ignite upon the application of a match to it in a saucer; but upon testing it he found it ignited very readily. He thought it worthy the consideration of this society how far petroleum oils might be employed for the purpose of heating steam boilers, instead of as a lighting agent. He thought the use of paraffin and petroleum oils highly dangerous with the present tall lamps, which were liable to be upset by the smallest accident, when the most serious consequences might ensue. He fully concurred in the views of Dr. Bachhoffner as to the desirability of a more satisfactory guarantee being given of the degree of inflammability of these oils.

Dr. Bachhoffner said petroleum had been used in America to a great extent for generating steam in steam boilers, and a report had been issued showing that it had been attended with certain advantages.

Mr. Wankyn thought too much importance was attached to the comparison of lighting materials as regarded the relative qualities of carbonic acid produced in combustion. Ordinary air contained from three to five parts of carbonic acid gas in 10,000 parts of air. Experiments made upon air taken from the most crowded theatres of London showed it to contain not more than fifty parts of carbonic acid in 10,000. If they considered the immense quantity of air there was in a room, and made a simple calculation, it would appear that no description of lighting material they could have could produce an amount of carbonic acid that was likely to have a serious effect upon health. He believed it had never been established that the difference between 5 parts and 50 parts in 10,000 had an injurious effect upon the health; and the probability was that the injurious effects of close air were due to something else, rather than to the slight additional quantity of carbonic acid gas. He believed the amount of carbonic acid present under such circumstances was practically of no moment whatever. With regard to the danger of explosion, petroleum had only recently come into use and he believed one of the most effectual ways of guarding against this was to use lamps holding only a small portion of oil. To cause an explosion they required air to be mixed with the vapor of petroleum, and it was only when this vapor was present in a large proportion that explosion was possible. When the lamp was full of oil there could be no explosion; the accidents occurred for the most part when the lamp was nearly empty.

Mr. Tegetmeier said, having investigated the circumstances of a great many accidents alleged to have been occasioned by the explosion of paraffin lamps, he had only met with one genuine instance, which occurred at Bethnal Green in the autumn of last year. In that case the lamp was a cheap and badly constructed one; the oil used was inflammable at 110° (disposing of the alleged safety of oil at 100°), and a woman in attempting to put the light out blew down the chimney, which ignited the oil, and the burning liquid was scattered over her, occasioning a considerable amount of injury. With regard to the inflaming point of oil he understood Mr. Paul to put the point of safety at 100°. He (Mr. Tegetmeier) thought the inflaming point should be placed some degrees higher. He had never traced an accident to oil inflaming at over 120°; therefore he agreed with Dr. Bachhoffner that they ought to have a guarantee of safety up to about 130°. As to danger in the use of really good oil, he believed it was practically *nil;* and with oil uninflammable up to about 130°; they might use the lamp in the roughest manner, and even spill the oil, without the slightest danger.

Mr. Paul, in reply upon the discussion, said with reference to the remarks of Dr. Marcet, as to the relative amount of carbonic acid given out by different lighting materials, it was only necessary to say that as the illuminating power of these materials entirely depended upon the amount of carbon they contained, it followed that for a given quantity of light they must yield a given quantity of carbonic acid. In the table given in the paper the various materials mentioned were

seen to contain carbon in slightly different proportions, the greatest amount being contained in olefiant gas, which was about 86 per cent., while marsh gas contained about 75 per cent., and the latter had no illuminating power, while the former produced a brilliant light. It was a mistake to suppose that petroleum had an advantage over coal gas in producing less heat: for the production of heat as well as of light depended upon the amount of carbon; the more light, the more heat, and the more carbonic acid they had. The only room for improvement in the case of gas, as far as he could see, was with regard to the diluting agents of olefiant gas; these were of three kinds—carbonic oxide, carburetted hydrogen, and hydrogen. These differed in the amount of heat they produced, and carbonic oxide was objectionable from its poisonous properties. The improvement would be to substitute hydrogen for marsh gas or carbonic oxide; still he repeated, for a given quantity of light they must have a given quantity of carbonic acid and a given quantity of heat. The production of carbonic oxide under the circumstances in which gas, petroleum or candles were burned, seemed to him to be beyond the range of possibility. Carbonic oxide could only be produced by the combustion of carbonaceous substances where there was a deficiency of oxygen. When a light was burnt with insufficient air the consequence was smoke, which preceded the production of carbonic oxide, and gave sufficient warning of the risk. There was a method of lighting which had been tried, which he had no doubt would eventually be successful, and would be largely used, in which the light was obtained without any carbonic acid being produced. He referred to the lime light, in which the solid substance, instead of being carbon, was a piece of lime, intensely heated by the flame of oxygen and hydrogen. The possibility of using that plan of lighting for ordinary purposes rested upon one circumstance, viz: the production of cheap oxygen. If this could be cheaply supplied there would be no further difficulty about the lime light: but under present circumstances the improvement that could be made in the existing methods of lighting consisted in better modes of ventilation, such as were adopted in this room—a principle first introduced by Faraday, and one which might be regarded as perfect in all respects. With regard to the Rangoon petroleum, the history of it was very simple. It was an extremely good material for the manufacture of hydrocarbon oils, the best of the kind being that known as the Belmontine, of Price's Candle Co., but the cost of Rangoon petroleum in this country was from £20 to £25 per ton, while the American was supplied at from £12 to £18. It did not therefore require many words to say why Rangoon petroleum was not used. With regard to American petroleum he was far from enthusiastic about the future of that. He was disposed to think the oil distilled from coal and other similar materials would become the staple material for the production of these illuminating oils. With regard to the alleged danger attending the use of these materials, he must say he could not agree with the apprehensions entertained by gentlemen whose opinions, however, were deserving of great respect. The idea of being limited in the use of these materials

by the stupidity of domestics was a thing he did not like to submit to. The probability of a gas tap being left with the gas escaping was quite as great as that of any blunder through which an accident might be produced by the use of petroleum. Of course in the use of all these materials a reasonable and proper amount of care must be exercised, and, as with ordinary gas, they must have proper apparatus for burning these oils, which apparatus must be carefully managed. The fact that these oils could afford light in country places, where gas was not obtainable, at a price of little more than half as much again as gas, was sufficient to prove that they were a considerable boon to the great mass of the people. There was another fact which was not sufficiently attended to—that was the enormous aggregate consumption of these oils. There were several factories which turned out 30,000 gallons of oil per week, and the number of accidents they heard of was infinitely small when compared with the extent to which these materials were used. At the same time it was quite proper that it should be settled by competent authority what should be the standard of safety in these oils. The Act of Parliament, passed some time ago, unfortunately only applied to the storing of petroleum and other inflammable substances, and left out of consideration other important points. The Act merely provided that not more than 40 gallons of oil, lighting at a temperature below 100°, should be stored within a certain distance of a dwelling-house; but less than 40 gallons of such oil, if ignited, might occasion very serious damage. With regard to the temperature of ignition, if it were possible to use oil that would light at 100° with perfect safety, provided they used proper lamps in a proper way, it was desirable that no impediment should be placed in the way of doing so; at all events, the question should be settled, and the absolute degree of safety established. With regard to the use of these materials for fuel, that admitted of very simple comment. He was aware they had been tried in America, but a slight consideration of the composition of petroleum would show that this was not likely to answer commercially. The heating power of petroleum was not more than $1\frac{1}{4}$ times that of coal, and while the price of petroleum in this country was about £18 per ton, coal was about £1 per ton. From this fact alone they might judge how far the application of petroleum to heating purposes in manufactures was practicable.

Mr. Symons said that the assertion of Mr. Paul—that heat bears a constant proportion to light, did not agree with the experiments of Dr. Frankland. According to the published tables of these experiments, the heat produced did not bear any constant proportion to the amount of light evolved.

The Chairman thought they could not but feel obliged to Mr. Paul for the interesting paper he had brought before them this evening, and also for the further observations he had been kind enough to make on matters arising out of the discussion. The subject of lighting was a very interesting and important one, both as regarded the lighting of towns and the interior of dwellings. Those who had been in any of the towns of the East, which were entirely unlighted at night, except

by the small lanterns people carried about with them, would not be surprised at the fact, that when the Turkish Ambassador came to London, he thought the city was illuminated in honor of his arrival, though the only lights used at that period were oil lamps. He might mention, that long after the introduction of gas into Newcastle-upon-Tyne, he saw a house there blown up by gas. The singular fact connected with this occurrence was, that in that house no gas was laid on; but the pipe of the public lighting having burst, the gas found its way into the house, and the servant, going about with a lighted candle, produced the explosion, which was attended with very serious consequences. That accident, at the time, was very likely to lead to a supposition that gas would prove a very dangerous thing, and that accidents of that kind would be of frequent occurrence. Yet it was the only instance of the kind he had ever heard of, and 40 years had passed over without the repetition of such an occurrence, showing how erroneous a view might be entertained as to the question of danger. Reference had been made to the application of these oils to heating purposes. That was a subject well worthy of most careful consideration, because everything that tended to economize coal must tend to the duration of this country as a great and powerful nation. The Chairman concluded by moving a vote of thanks to Mr. Paul.

The vote of thanks having been passed,

Mr. Paul, in reply to what had fallen from Mr. Symons, said he thought Dr. Frankland would be the last person to dissent from the view he had put forward as to the amount of light, and consequently of heat produced, being in direct proportion to the amount of carbon contained in the vapor burnt. As to the reason why the oxyhydrogen blow-pipe did not give light in proportion to the heat, it was simply from the fact that the gases burnt contained no carbon, and yielded no solid product.

Artificial Lighting.

From the Journal of the Society of Arts, No. 504.

The observations I made in the discussion on Mr. Paul's paper, on the 1st inst., were in reply to his statement—"It was a mistake to suppose that petroleum had an advantage over coal gas in producing less heat, for the production of heat as well as of light depended upon the amount of carbon; the more light the more heat;" "for a given quantity of light they must have a given quantity of carbonic acid and a given quantity of heat." There appears to be a misapprehension of the generally received theory of the correlation of physical forces in these assertions; a deffinite quantity of force will no doubt be given out in the formation by combustion of a certain quantity of carbonic acid, but the following table, from Dr. Frankland's lecture at the Royal Institution, in February, 1863, (on which my remarks were founded,) shows that this force may be given out in largely varying proportions of light and heat. In connexion with the subject under discussion, the table may be otherwise interesting to some of your readers who may not have seen it. It shows the amount of car-

bonic acid and heat generated per hour by various illuminating agents, each giving the light of 20 sperm candles.

	Carbonic acid.	Heat.	Cost. s. d.
Tallow,	10·1 feet,	100	2 8
Spermaceti,	8·3 "	82	6 8
Wax,	8·3 "	82	7 2½
Paraffin,	6·7 "	66	3 10
Coal gas,	5·0 "	47	0 4½
Cannel gas,	4·0 "	32	0 3
Paraffin oil,	3·0 "	29	0 5
Rock oil,	3·0 "	29	0 6½

It will thus be seen, at least according to Dr. Frankland's experiments, that for exactly the same amount of light, tallow emits nearly four times as much heat as paraffin oil, and more than twice as much as coal gas.

W. Symons.

For the Journal of the Franklin Institute.

Atmospheric Pressure as a Traveling Force, versus *Animal Power and Steam Locomotives.*

Steam is prohibited from driving cars through this populous city. It could not be admitted without a sacrifice of life, hence horses on our street railways. Time is most appreciated in commercial communities. Almost every one entering a car wishes to be at the end of his route in the shortest possible time, but that cannot be attained or even approached in streets so generally thronged and sometimes blocked with carriages. Twelve passengers is the complement of an omnibus, but rail cars are "never full," though jammed inside and out they stop for every applicant and leave him to find standing room where he can. As on some other lines, the Fourth Avenue cars start every two or three minutes. They run from below the City Hall to 32d Street—nearly 2¾ miles. The time given to the conductor is 30 minutes, each way. The actual average is I believe two or three more. "Add to the number of cars" is the natural suggestion and it will have to be done, though it will add to the number of stoppages and dead-locks at crossings. Soothing one sore it will aggravate another. A few *through* lines would do something but where is there room for them. Increased and uninterrupted speed is the only remedy and it is wholly impracticable, and inadmissible if practicable. The business part of the city is so crowded that stoppages and quarrels of drivers are almost constant. The carmen complain through the press and formally to the city authorities of rail tracks which, they say, their carts cannot travel on without injury to their horses. The feeling may be inferred from remarks of the *Tribune* of June 24. As an historical fact the extract is worth preserving.

"There was another hearing of complaints against city railroads yesterday, before the Common Council Committee. The question is easily narrowed down to a simple proposition: Are the streets of New

York for the whole people or for the carmen? If for the people, the carmen—an integral portion of that people—must take their chances with all the others. If for the carmen, the people must be delayed, obstructed, annoyed, run over, as they are now to a certain extent, and permit their business to suffer for the accommodation of but a mere fraction of their own number. Carmen complain that rails break their carts; then let them keep off the track—they cannot get more than one wheel on at any rate. There is room enough in almost any street to keep clear of the rails. If they have not a load to deliver in a street having rails, there is no good reason why they should prefer that street; if they have business there, they need not drive on the rails; if they necessarily back up across the track, the cars must wait, and the railroad men do not complain, however passengers may grumble if they will insist upon driving on or across the track out of pure spite, as they very often do, to our certain knowledge, they are properly ordered off by the police. We do not say that car-drivers and conductors are models of patience, but we know that it is the especial delight of a great many carmen, stage-drivers, grocery men, fish and vegetable peddlers, and all other divisions of the carrying trade, to delay, thwart, annoy and insult the men who manage cars—we know that some of them deliberately seek occasions to be annoying, and have often suffered until almost out of patience with their bullying."

Looking beyond the present hour we apprehend a system will have to be adopted that will neither require horses nor interfere with street travel. For want of room on the surface the lines will have to be subterranean or aerial—under or over the houses. At present, decided objections would most likely be urged against both. But, passing that, suppose it were possible to inaugurate a system by which a Fourth Avenue car-load of passengers could be sent through in three minutes, that would be equal to ten of the present trips, and if double the number of passengers were taken it would be equivalent to twenty. That is, work would be done in 30 minutes which now requires 600. In other words 200 trips would be made in ten hours instead of 20: and with a single car instead of a dozen.

Now, as a general proposition, this is both possible and practicable. Still, not to draw too largely at once on public credence, let those who think it too utopian, double the time and say six minutes a trip instead of thirty—less than a mile in two minutes. To some minds even that may seem, if not visionary, in advance of the age and of the arts; but, in reality, it is in advance of neither. Enterprise and intelligence can realize it at once. If too late, or too early, to adopt it here it may not be elsewhere. The main point has already been established. The principle is that of the abandoned *Pneumatic Railways*, with this difference that passengers are to be carried through the tunnel or tube instead of over it. Don't be alarmed. When duly considered, objections will be found more imaginary than real.

"Why, must not blackness of darkness pervade the interior?" Not necessarily, since gas or oil lamps may be carried along as in night cars. But the air in crowded cars is offensive in hot weather; must it

not in such tunnels resemble that in the black-hole of Calcutta? No, not in the longest line. No space could be more thoroughly ventilated since fresh air would be constantly streaming in, besides sweeping through at every stopping place. But as passengers are always moving with the piston away from the mouth, how is fresh air to reach them and displace that which enveloped them at starting? Easily; the windage of the piston would, or a valve could, be made to accomplish that perfectly. No air unfit to breathe could accumulate. Then, where is the difficulty of having sun-light by day as well as lamps and candles by night. As the tunnels require no connexion with exterior mechanism, instead of being buried in the ground they may be laid upon it (and where required supported above it,) by the sides of common roads, over fields, or through forests, as circumstances may suggest, and to have plates of glass in the roof and sides. Thick slabs of that material are now used on store floors to transmit light below. I see nothing impossible in the idea that atmospheric tunnels may ultimately be formed chiefly if not wholly of glass instead of iron, romantic as it may now appear.

There are no more limits to the length of atmospheric lines than to railways. Landing and receiving passengers at intermediate stations present but little more obstructions in one case than in the other. At each station a hinged iron door fitted air-tight to its frame (cast on one side of the tube) gives entrance and egress. But does not that involve the loss of the existing vacuum in the untraveled portion? Not at all. The piston must pass the door before it can be opened or the passengers reach it. It has then only to be anchored; and freed as soon as the change of passengers is made. The officer at the station may then either close the door or leave it open till the return trip begins. It would require no fastening inside or out.

The rate of travel will of course depend on the extent or degree of the vacuum, and that on the power of the exhausting apparatus. In the Pneumatic Railway from London to Croydon, about nine miles, the pressure was eight pounds to the square inch and the velocity 30 miles an hour with a train of sixty tons. What would it have been with ten tons and a tube without a slit through its entire length? While some birds fly 60 miles an hour and some, as frigate birds, at a higher rate against the air, there is little risk in saying that passengers may be blown by it through vacuum tubes at a greater speed, and with emotions as bland as those which an agreeable breeze imparts; or with feelings so slightly unpleasant as to border on pleasure.

A Pneumatic Despatch Company has been recently established in London and mail-bags are being sent through air-tight tunnels. Occasionally a couple of attendants have passed through "without the slightest discomfort." A London Journal says, "the next step of the company will be to lay tubes connecting the markets with the Cambden Goods Station, with a tube to the General Post-Office and Pickford's depot in Gresham street, and these operations will eventually tend to *revolutionize the carrying system of the metropolis, and relieve the crowded state of our principal thoroughfares.*"

Apply the principle to passenger traffic and how much greater the results.

As the connexion between morals and mechanism is inseparable, Humanity has something to say on the subject; and though self-interest may be deaf to its teachings when they conflict with gain, there is no better listener when they harmonize with increased profits. There are two safe criteria for judging of inventions. Every good one is a contributor to morals, direct or indirect: a device that lessens the demand on living force is an example of the former, and that which saves time, of the latter. When air-lines become common we shall cease to witness the noblest of quadrupeds prematurely worn out and dropping from exhaustion in the streets.

It may be presumptuous to augur that steam locomotives and their trains will ere long fail to meet the demands of society; but it would be much more so to conclude that they comprise the perfection of mechanical resource, and are to be the *ne plus ultra* of land traveling for all time to come. In nature nothing is born or matured at once, and in art nothing springs complete from human brains. Inventions, like plants, are designed to grow till they are ripe, and, when all is got out of them that can be, to give place to others. When one has fulfilled its promise in the front ranks of civilization it is to fall back till it reaches the hindermost. Ages may elapse before this is done, or can be done while barbarism endures. Flint implements continued in use through ages of bronze, and iron has not yet reached all savage tribes.

That the rapid transit of men and merchandise is an important, and destined to become a leading element in civilization, no one can doubt who observes its influence on the social, political, commercial, and intellectual world. If it stand still, progress in other departments must be arrested. Whatever may be the extreme limits to speed, there is little risk in asserting that the present mean rate will in time be deemed intolerably slow. It is not every one that is now satisfied with it, and the number is not small who wish it were doubled.

When faith in progress becomes general, improvements in the arts will become common. The more there is of it the less of that depressing unbelief which meets new projects, sometimes with derision, always with suspicion, and against which valuable novelties have to struggle into life. Preferring to rest satisfied with things as they are, it is doubtless ready to ask what possibility there is of any marked advance in traveling by steam, and where the necessity for it? The very objections to mail coaches in the last century, which were to pass over roads good and bad, night and day, at the incredible average rate of ten miles an hour: to the first steamboats also, which if they did not threaten to upset and break the limbs of their passengers, would subject them to the double risk of being scalded to death or blown piecemeal into the air.

Had the leap from ten miles by horses, to fifty by steam, been attempted at once, it would have staggered the boldest, and probably have brought public execration on the proposer; but nothing of the kind can take place. By the law of progress, improvements are gradual —never precipitate.

The timid would not willingly be now among the first to travel at the rate of 80 or 100 miles an hour, if they even had little fear of its taking away their breath. The feeling is a natural one and therefore to be respected, although it has no rational foundation. It is chiefly ascribable to ignorance of the fact, that the highest speed no more affects our bodily organs than the lowest.

Passengers in the cabin of a ship flying before the wind, have no more sensation of going forward than when she is lying at anchor. A balloon rushing upward, or in a lateral direction, appears to the aeronauts stationary and motionless. In night rail trains we walk to and fro, sit and sleep, unconscious of progression as in a parlor or bed-room. In day-time it is the same if we close our eyes to objects outside. Jolts from obstructions and irregularities of roads, with changes of direction and diversities of speed, tell us we are moving. In a perfect system of travel there could be no sense of motion at all, whether the rate was one mile an hour or a hundred, or five hundred. And even that is a snails pace when we look, and we ought often to look, beyond our petty doings to those of the Great Engineer. Our earth is one of a line of passenger cars that conveys us through space at a mean velocity of 68,000 miles an hour, without disturbing a loose brick on a chimney, or displacing a grain of dust, proclaiming the fact that uniformity of motion is equivalent to rest, and suggesting that as with time and eternity, heat and cold, light and darkness, attraction and repulsion,* &c., rest and motion are one. Hence if the time should ever come when 1000 miles an hour can be done, old ladies and young children will be no more inconvenienced than now, and in all probability not near as much.

But what are the main defects, or deficiencies, of the railway system, which make their removal or mitigation already a desideratum? 1. By a certain law the speed is confined within limits which cannot suffice to meet the requisitions of a progressive world, and, 2. It is not at all commensurate with the power expended. A train of cars meets with a resistance from the air that increases with the speed. What it is may be deduced from the force of wind against bodies at rest. When blowing at the rate of 20 miles an hour the force is nearly 2 lbs. on the square foot, at 25 miles over 3 lbs., at 35 miles 6 lbs., at 50 miles 12 lbs., at 100 miles nearly 50 lbs., &c. The cost of power for thus merely clearing the way for a train is therefore very great; but there is an outlay still greater which yields no return, and in itself sufficiently demonstrates that mechanical science is at an incalculable distance from its progressive terminus. Every observer knows how large a portion of the strength of draft animals is expended not on the loads but on the vehicles and drivers. In the weight of carts, drays, and omnibuses, the largest portion of the driving force is unproductive. In steam trains it is believed that with every ton of passengers not less than two tons, some say three, of wood and iron, fuel and attendants, &c., are borne along. That is to say, *two-thirds of the power* is *con-*

* See Cursory Thoughts on Natural Phenomena and the Unity of Force. New York, 1863.

sumed to make one-third productive, and less than one-third when light loads are carried. Though steam cars are not dragged hither and thither to collect loads, there is always a waste of power before starting and after stopping.

The wear and tear of locomotive engines is great and the cost of working them enormous. Their liability to be thrown off the tracks, and to collisions is constantly enforced on us by current accounts of such costly accidents.

The advantages sought for in the atmospheric system are therefore, greater speed and less outlay of working power—no "rolling stock."

The leading requisition is rapid transit, and for all practical purposes the velocity of a piston rushing into a vacuum is unlimited. At all events, something approaching to 100 miles an hour may, we presume, be attained with equal safety and a smoother motion than by open rail cars. Jolting or jarring would be reduced to a minimum. The passage through the tubes might be on runners instead of wheels, At first nervous persons would be haunted with fear of traveling in what they would call a close tube. But would it be a close one? Rather consider it a vaulted passage with sky lights and windows, and (besides openings to get in and out at stopping places), whose back door remains open till you reach the front one. Granting that it may not be to every one's taste, what is there that is? It certainly offers some advantages over railways which travelers know how to appreciate; as freedom from rain and sleet in winter, wind and dust in summer, and smoke (no trifle) all the year round. Add security against running off tracks and against those terrible collisions so awfully destructive of life and property that continue to occur, and the most timid will be ready to say, "better a close passage free of these than an open one exposed to them.

The speed may be increased from the slowest movement at starting, or be uniform throughout by exhausting the air before starting. In the latter case, there would be a saving of time and power, for the moment one load of passengers arrived, the force to send back another would be accumulating. That is, while a locomotive engine or horses are waiting till a train is ready to move, the power has done most of its work, and has only to finish it by drawing out the remaining air as the piston moves on.

In the simplicity of its mechanism and saving of power the atmospheric system is one of great promise. The power is expended more on the load and less on the vehicle than in any other. There are no massive locomotives and tenders to generate and carry the force to and fro, and in so doing unprofitably consume most of it. On the contrary the motive engines being stationary not a fraction can well be thrown away on the exhausting apparatus, whether a load is in transit or not. Then as there is no resistance in front of a load of passengers the speed increases directly with the force. There is another advantage that deserves more than common attention. In the act of withdrawing the resisting air in front of a load passing through the tube, Nature promptly and without cost furnishes the propelling power

in the rear; whereas steam locomotives *gain* nothing by overcoming the opposing fluid. The difference is quite as much as between leading a willing animal after you and forcing a resisting one before you: the more you hurry it the more obstinate and refractory it becomes.

These remarks are thrown out with the hope that they may lead those interested in contemplated new Lines of Railroads to inform themselves of the capacities of the atmospheric system. It is unnecessary to enter into details since most of them have been anticipated. The abortive Pneumatic Railways established the principle completely, and what if it failed in one form of application is that a sufficient reason in this day of great things not to try it in another? If projects not half so full of promise as it is, had always been so treated, the world would not be what it is. Thus tamely to abandon a world-wide desideratum on account of a mechanical difficulty, and not perceiving the lesson that difficulty taught, will never be sanctioned by those who believe Atmospheric pressure is destined to take its place among popular motive powers, and therefore fitted to do what others cannot do, or in all respects do as well. Every force can do something better than another, and it is the business of the engineer to find out what that is.

No mechanical experiment is without good results, negative or positive; and the one referred to, in showing that the load must be passed through the tube instead of over it, will repay to society more than all it cost: not yet perhaps in Europe if the testimony of a recent writer is reliable, viz: that it is there generally conceded that atmospheric pressure is of no use to the business of railways except as a substitute for ropes to pull trains up grades too steep for locomotives. Had a moiety of the money and ingenuity laid out on the attempts to connect the piston with the trains by a factitious, and practically impossible, device been applied to illuminate and otherwise fit up the tunnels for the conveyance of passengers, that opinion had not been hazarded nor endorsed.

The data for determining the comparative economy of the two systems must be wide of the view we take of them, if the expense of working atmospheric be not *one-third* less than that of locomotive lines. The first cost may be greatly in favor of the latter and yet the difference may not be so great as imagined. The durability for centuries of a line of tubes must be taken into the account and the less amount of grading required. Ascents and descents on medium undulating ground would require little or none. It is not however what a line costs but what it earns, and the amount to be deducted from its earnings for working it. That is the main thing to be considered, and it is that which seems to render it all but certain that the atmospheric system is destined to accompany if not to supplant the other.

I am aware that the development and progress of the arts are as much under law as natural productions; that generic inventions appear in a regular sequence, that they can no more come out of their order than living genera, that before society is prepared to receive them they would be deemed worthless abortions of which nothing could be made; and that if it were possible to learn what discoveries in, and applica-

tions of, science are to characterize remote cycles of time, they could in all probability be little better appreciated by us than leading achievements of our day could have been ten centuries ago. The future is not to be anticipated in its products any more than in itself, in its philosophy than in its philosophers, in its inventions than in its inventors. All therefore that we have to do, or can do, is to extend and improve the applications of known principles and forces, and in doing that we open the way for and hasten the advent of others.

Although things below the mental are as invisible as those beneath the material horizon, fresh additions to the arts may generally be found by marking the signs of the times. There are always some ready to be adopted. As respects improvements in travel, only one has risen fairly into sight—the atmospheric system. There may be others more promising, open to clairvoyant engineers but we should rather deem them such as are casting their shadows before them, and not likely to emerge into full view for some time to come.

That such will come is certain. Indications of progress everywhere abound, and as respects engineering, and the arts generally, in a quarter the most significant. Of the *metals*, iron has been and is justly esteemed the most valuable and indispensable. The prime agent it is also the measure or standard of civilization. People rise in the scale in proportion as they employ it. This, its destined influence, is denoted by the fact that it occurs more abundantly than most others, and more than all or nearly all of those in common use put together. Its pre-eminence was early recognised and is now more fully conceded than ever. Without it barbarism and semi-barbarism would still have the world to themselves. Without it where had been steam engines, rail roads, gas, and water works, telegraphs and other ancient and modern achievements. They could have had no existence but in the imagination and possibly not even in that.

But passing what it has done and is yet to do, is its glory to be eclipsed? Or are others to be subject to it and their applications to depend upon it for ever? If yea, human advancement is certainly limited; and the end may be thought to be almost in sight, since the properties of this metal are pretty thoroughly known and to a considerable extent made the most of. Its consumption may be vastly increased but progress implies more than mere consumption. As it interposes serious checks to the extension of engineering, as in beams that bend and chains that break with their own weight, is it to be inferred that the application of metal to these and kindred purposes has reached the climax? Decidedly not.

It is now ascertained that metals exist in greater profusion than iron and with properties that will make them surpass it both as regards the magnitude and variety of its applications. Two, of three most abundant, are *aluminium* and *magnesium*, the former three times and the latter four times lighter than iron, and rivaling in their alloys steel in hardness and tenacity. One is partially and the other wholly free from the cancer to which iron is subject—oxidation from exposure to the atmosphere. These are the very qualities wanted. What could

not now be accomplished even in rapid traveling if locomotive engines and their trains were reduced to one-third of their present weight, and two thousand ton steamers to five hundred tons. What heavy and weak affairs our chain cables, suspension and other iron bridges, &c., will one day appear in contrast with the neat, light, and lasting fabrics then in use. But everything—every metal—in its own order.

E.

New York, June 29th, 1864.

On Fibres for Brushes.

From the London Mechanics' Magazine, July, 1864.

There is a great demand just now among brush-makers for some strong supple fibre which may supplement the present supply of piassaba or *bass* received from Brazil. Two strong fibres, the produce of different palms, have been for some time received from the two ports of Para and Bahia. Of late less care has been given to the selection and preparation of these fibres in South America, and they are much mixed with waste and useless fibre. The piassaba fibre, and the so-called Mexican grass, the produce of the leaves of Agate, have quite revolutionized the brush trade by cheapening the cost and replacing bristles. Owing to the improvements in Russia, arising from attention to bacon and ham curing, bristles are less stout and plentiful than they used to be from the wild hogs. The attention of residents in tropical regions may therefore be drawn with advantage to the demand for new fibres for brush-making, street sweeping machines, and chimney cleaning brushes. The midribs of the leaves of many of the palms, a stronger and stouter substance than the kittool and ejoo fibres of Ceylon and the East, are what is wanted, and probably some wiry grasses like the esparto might be experimentalized upon with advantage. They would realize a good price if suitable and to be obtained with regularity and in quantity.

Deposit in Boilers.

From the London Mechanics' Magazine, July, 1864.

SIR:—As you requested me to inform you how I got on with my experiments on removing the deposit from steam boilers with ordinary steam, I beg to inform you of the result.

I had the boilers blown off on Saturday evening, and on Sunday morning at six o'clock I commenced letting the steam into them. I kept the supply of steam up to eleven o'clock, with a slight flame from faggots both in tube and flue; I then opened the manholes. The men went in on Monday, when they succeeded in removing about one-third more deposit than had ever been got out before, although the boilers were working a week less than their usual time. I also find we are doing the same amount of work since with a slight decrease in our consumption of fuel.

By taking these things into consideration, I am inclined to believe

that we have done some good, although upon going into the boilers I could not see much difference in their condition. Still it is my intention to try the process every time we clean out, which is about once a month, and I am sanguine that eventually the boilers will become thoroughly clean.

It is my intention to use the soda ash, as you suggested. I should be very much obliged if you would inform me what quantity I ought to use. In my former letter I gave you the dimensions of the boilers. Our consumption of coal is about 1 ton 10 cwt. per day.

Process for making Shot.

From the London Mechanics' Magazine, July, 1864.

Captain Palliser, of the 18th hussars, has produced strong and exceedingly cheap shot by a simple process. Instead of casting the shot in sand and allowing the surface to cool gradually, the metal is poured into a cold iron mould so as to cool the surface with the utmost possible rapidity long before the interior has hardened. In this way a ball is turned out which, to judge from recent trials, combines almost the hardness of steel with the destructive effects of a segment shell.

For the Journal of the Franklin Institute.

Description of Messrs. Borie & Mackie's Cork Cutting Machine. Patented September 29th, 1863.

On reference to the accompanying cuts, C, C, C, C, are parallel cross pieces of a frame work in front of which revolves the vertical continuous knife, A, the latter being secured to the edge of a horizontal disk, B. In the rear ends of the cross pieces turns a shaft E, to which are secured arms F, the latter being connected by rods *e* to a frame D, which slides in slots *a* in the cross pieces C; the journals of the shafts *c* and *c'* turning in the said frame. On each of the shafts *c* is a toothed pulley J, which engages with a smaller pulley J' on the shaft *c'*.

To an arm *d*, projecting from the side of one of the cross pieces C, is hinged one end of a plate H, the other end of which is attached to a rubber spring *o*, the latter tending to draw the end of the plate towards the cross piece; and in the said plate is a slot *f* enlarged at each end, through which projects the outer end of one of the shafts *c'*, there being a collar *i* on the said shaft, of such a size that it can pass freely through the enlarged ends of the slot, but not through the smaller portion of the same.

A spring *m*, coiled round the shaft *c'*, is attached at one end to the frame D, and at the other to a collar *n*, which turns freely but cannot move laterally on the shaft.

In suitable bearings at the under side of the cross pieces C turn the journals of two driving pulleys I, which are so situated that when the parts of the machine are in the position shown in Figs. 1 and 3,

the peripheries of each of the driving pulleys will be in contact with that of the pully J above it.

When the machine has to be used, a rotary motion in the direction of the arrow is imparted to the knife A and to the driving pulleys I; a block of cork is then inserted, in the manner described hereafter, between the disks, *k k* and a vibrating motion is imparted to the shaft E. The first movement of the shaft E and its arms F in the direction of the arrows (Fig. 1) will move forward the frame D and the shafts *c* and *c'*, the peripheries of the pulleys J will be brought in contact with those of the revolving pulleys I, and a rapid rotary motion will thus be imparted to the shafts *c* and *c'*, and to the block of cork, which, as it is brought in contact with the knife A, is rapidly cut to a cylindrical form.

As the frame D approaches the limit of its forward motion, the collar *i* is brought opposite the enlarged portion of the slot *f*, and the plate H is carried over the said collar, and brought against the side of the cross piece C, by the action of the rubber spring *o*. As the frame is drawn back the collar *i* will bear on the outer side of the inclined plate H and the shaft *c'* will thus be moved laterally so that the disk *k* will cease to bear on the block of cork, which will fall into a suitable receptacle.

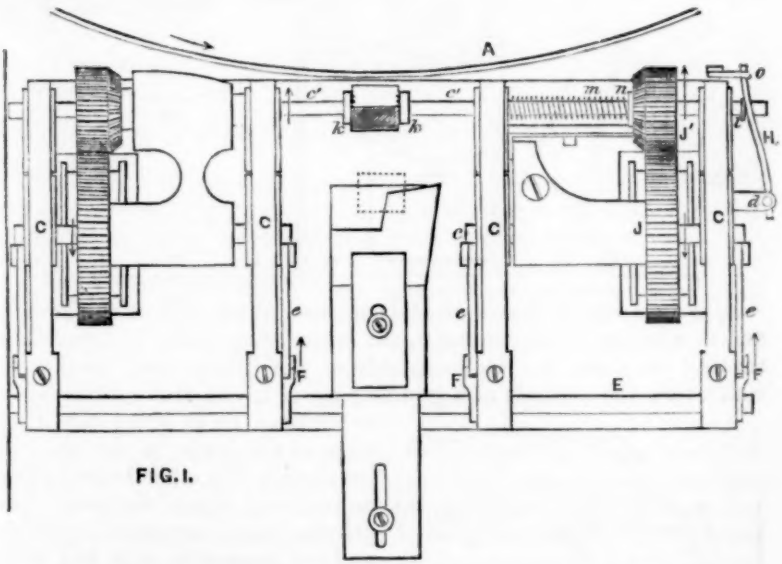


FIG. I.

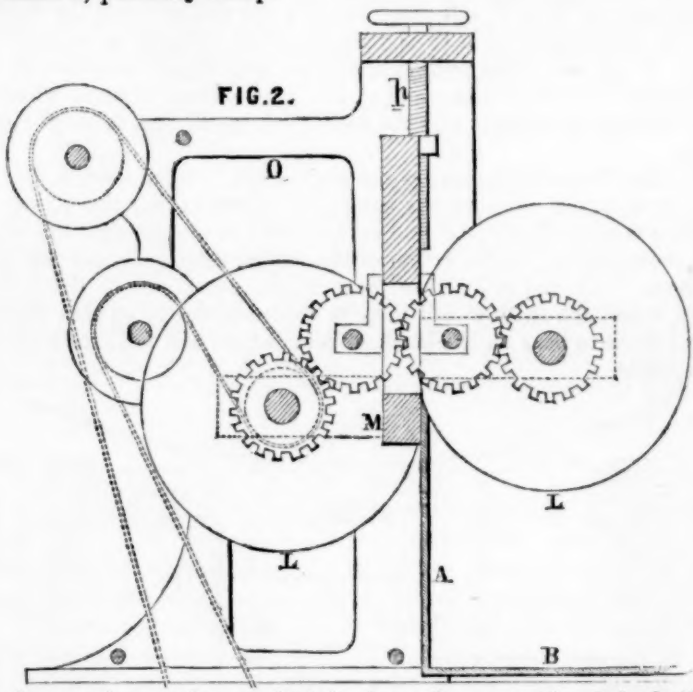
Another block of cork is now placed in the position shown by dotted lines, Fig. 1, and as the frame D is drawn still further back, the distance between the disks *k* is increased until each is opposite one end of the cork, when the collar *i* will reach the enlarged end of the plate H and will pass through the same as the spring *m* draws back the shaft *i*; the disk *k* on the end of the shaft is thus brought to bear on the cork, the

latter being held firmly between the two disks and carried forward to be operated on by the wheel A in a manner similar to that already described.

When desirable, several of these feeding devices may be placed around one cutting wheel A.

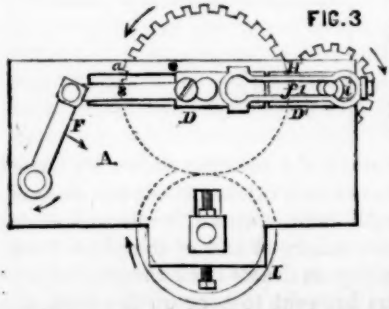
In Fig. 2 is illustrated a device for maintaining the cutting edge of the knife A, perfectly sharp.

FIG.2.



O, is a stationary frame which is situated at one side of the knife A, and in which slides the frame M, the latter being raised or lowered by turning the screw *p*. On each side of the frame M are brackets in which turn the journals of a grinding wheel L, one of the latter being so situated as to grind the inner edge of the knife to the required bevel while the other bears lightly on the outer edge of the same. The grinding wheels are geared together, and are connected with any suitable operating part of the machine so as to be kept in constant rotation. As the edge of the knife is ground away, the grinding wheels are maintained in contact with the same by lowering the frame M.

FIG.3



Messrs. Borie & Mackie have now in operation a machine in which

there are three feeders and a sharpening device, situated at equal distances around one cutting wheel. An attendant is stationed at each feeder which will feed over eleven thousand corks per day of ten hours, in which time, therefore, 33,000 corks are cut by one machine with three feeders, all of the corks being as smooth and regular as those cut by hand. By setting the feeding device at an angle to the knife, the corks may be cut of any desired taper.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, September 15th, 1864.

William Sellers, President, in the chair.

John H. Cooper, Recording Secretary, pro tem.

The minutes of the last meeting were read and approved.

The Board of Managers and Standing Committees reported their minutes.

Mr. Edward Brown, exhibited and described a model of a new manner of working the Slide Valves of Steam Engines.

Mr. Thomas Shaw described a new method of preventing the priming of Steam Boilers.

Mr. Lorenzo Vance exhibited a model of the apparatus for dressing Ship Planks, patented by him, which he described, and explained its advantages.

Professor A. L. Fleury read the following paper which he illustrated by diagrams.

The subject I have the pleasure to explain this evening is one that has recently occupied many of the best European metallurgists, and is one of particular interest to this great iron-producing State.

The want of a uniform and rational system of blast furnaces has long been felt, and judging from the reported results that have been obtained in the use of a new "Universal Blast and Smelting Furnace," the invention of the Russian Mining Engineer, and Major General Woldemar Rachette, this want seems to be fully supplied.

After three years of most perfect success in Russia, where a number of large furnaces have been built at the Iron and Copper Smelting Works of Prince Demidoff at Nischne Tagilsk, at the foot of the Ural Mountains, under the superintendence of the Mining Engineer, Mr. Charles Aubel, these results became known in Germany, and Mr. Aubel then undertook the practical introduction of this fully tested invention in that country. A furnace was at once constructed under his superintendence at Mulheim in Saxony, and the results which crowned this undertaking relieved every doubt as to the working of poor iron ores and the general advantages that have been claimed by the inventor, who has not, as is generally the case with important discoveries, been led to adopt at once his new system; it has taken him many years of costly experiments with various forms, such as annular, elliptical,

trapezoidal and horse-shoe-shaped furnaces to develope this last successful modification. Models of his system of smelting furnaces for working iron, copper, lead, and silver-ores, together with the products and the authenticated statements of the advantages that were obtained in Russia over the furnaces of the old system were sent to the great World's Fair at London, and the inventor rewarded with the prize-medal of that department. Letters Patent for this useful invention were granted in Russia, England, France, Belgium, Sweden, Austria, and recently also in the United States.

To give to the members of the Institute a full explanation of this invention is the object of my present communication. This system of blast and smelting furnaces differs from that now in use in several distinct, and very important features, and in order to give a correct idea, I deem it necessary to enter into a more detailed description.

The inventor has called this furnace "*Universal*," because the experience of several years has shown him that with but a slight modification of the general system it can with equal success be adopted for smelting of iron, copper, nickel, tin, zinc, lead, silver, gold, and other ores or mats, also for the direct production of cast steel, and the liquefaction of metals in general.

The *hearth* of the Rachette furnace (as seen in the ground plan of the annexed sketch taken from the working drawings of the last built iron blast furnace at Mulheim), presents the shape of an *elongated rectangle*, and rises up in the form of an inverted square pyramid.

The *shape* of the furnace, *long*, *narrow*, and comparatively *low*, is further distinguished by being *wider* at the tunnel head (the top of the furnace), and having a double series of tuyeres, (blast tubes,) arranged in alternate, interchanging order.

The iron blast furnace has *two working sides* or tymps, and under its hearth and all through the walls *a system of channels and flues*, by means of which the furnace, *while being built, is dried out and heated from the outside to the inside*, and afterwards *ventilated*, in order to preserve the walls from being too much expanded and injured by the accumulating heat.

The advantages developed and obtained by these radical features of the Rachette furnace, compared with the results of the present furnace system as stated in the European journals may be summed up as follows:

I. *A great saving* (in some instances *of more than one-half*) *in material, time, and labor in their construction.*

Mr. Aubel states that the cost of a Rachette furnace (of 2600 cubic feet area) at Nischne Tagilsk in Russia, including the tunnel bridge and furnace-mantle, was only 3500 silver Roubles (75 c. in gold each) and estimates that a similar furnace, producing about 20 tons of cast iron per day costs in Germany not more than 10,000 *Prussian thalers*.

At the present high prices of labor and material in this country, I think that a Rachette furnace of 20 tons per day capacity (exclusive of blast, and other machinery), can be built for from 15 to $25,000.

The last constructed furnace at Mulheim, Germany, producing from

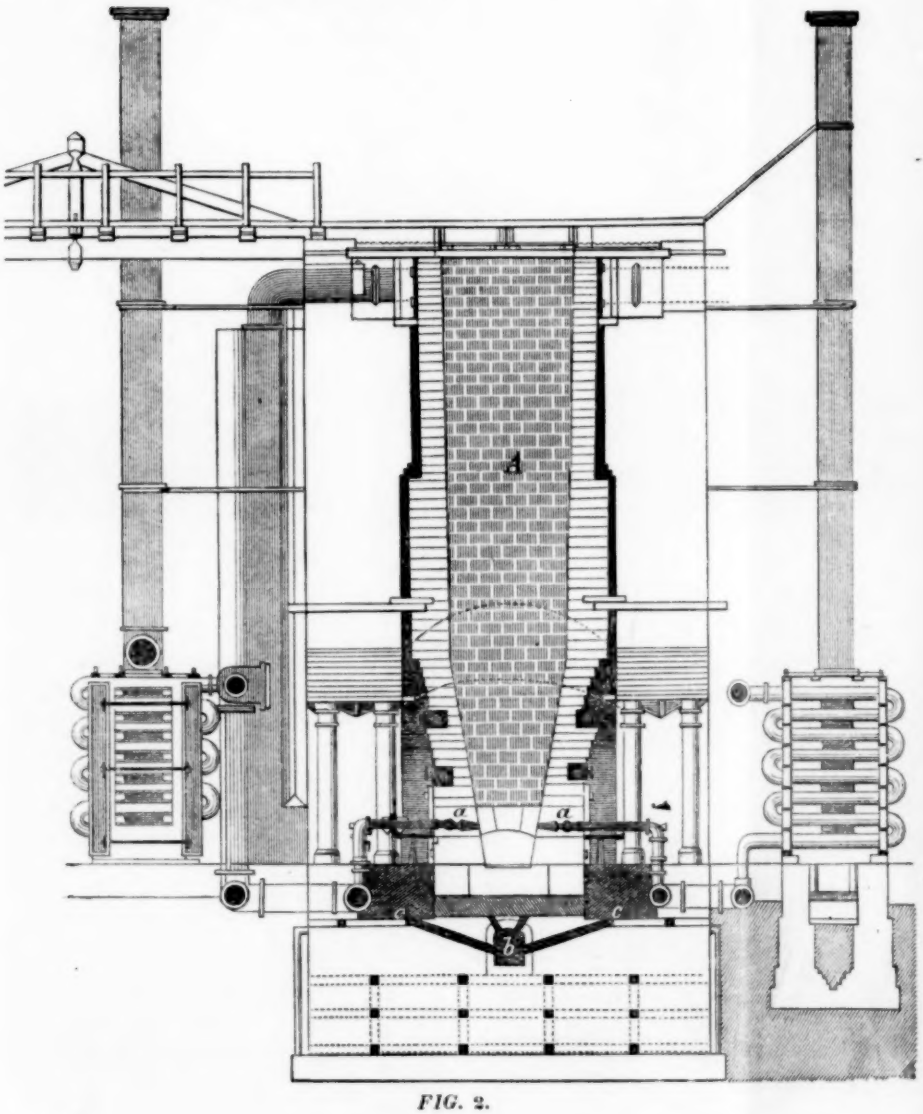


FIG. 2.

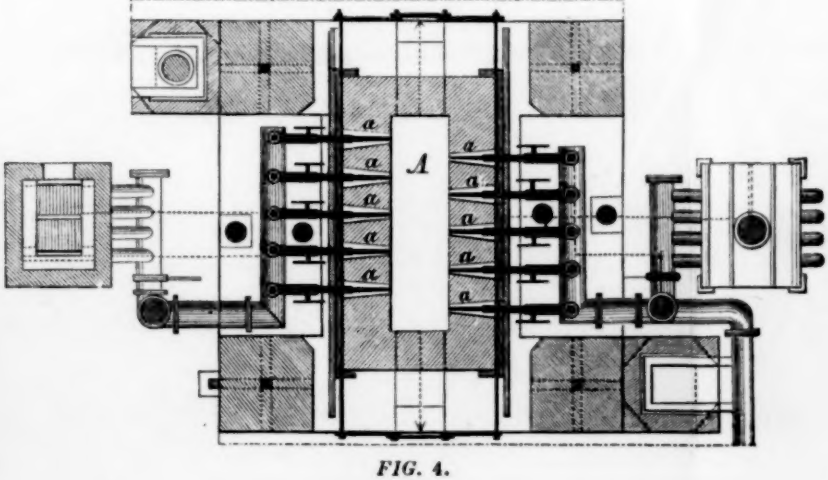


FIG. 4.

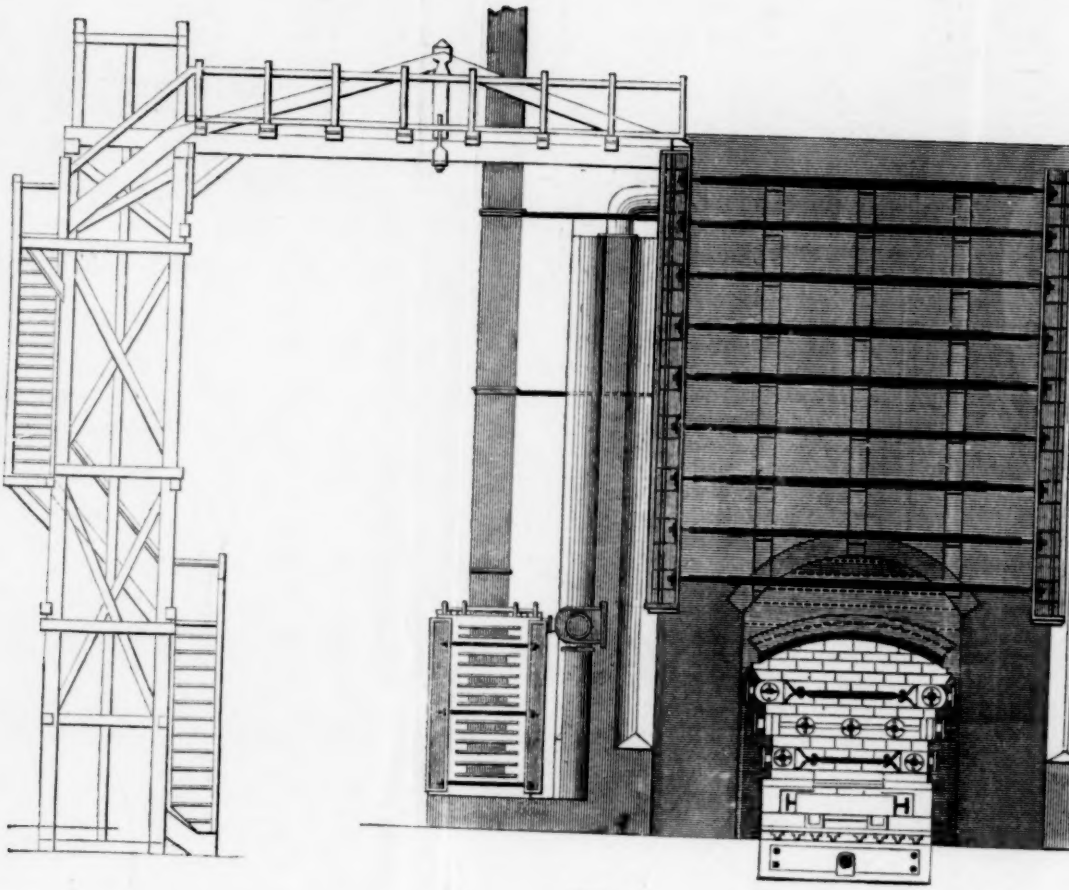
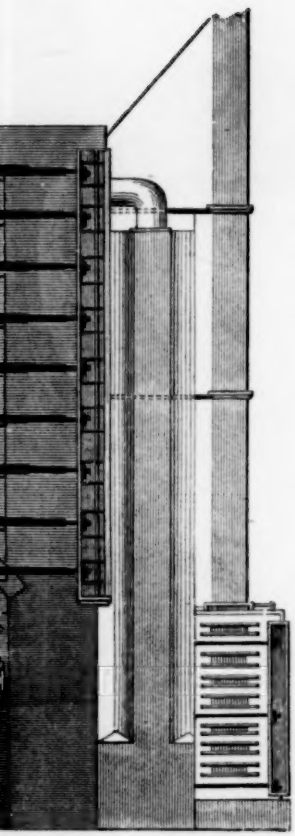


FIG. 1.



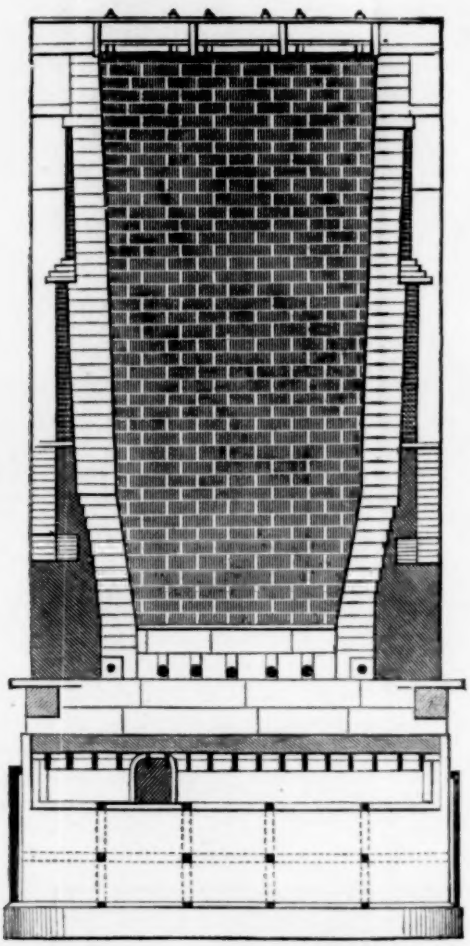


FIG. 3.

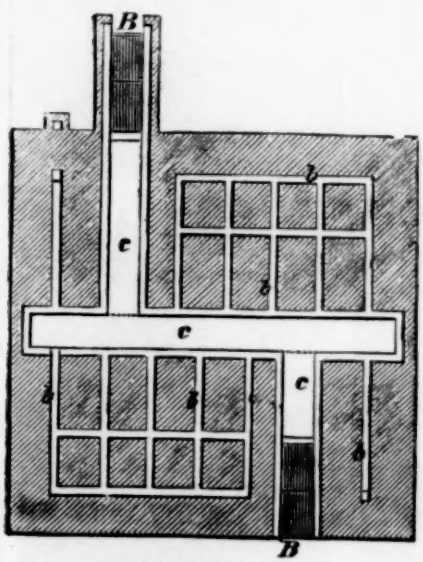
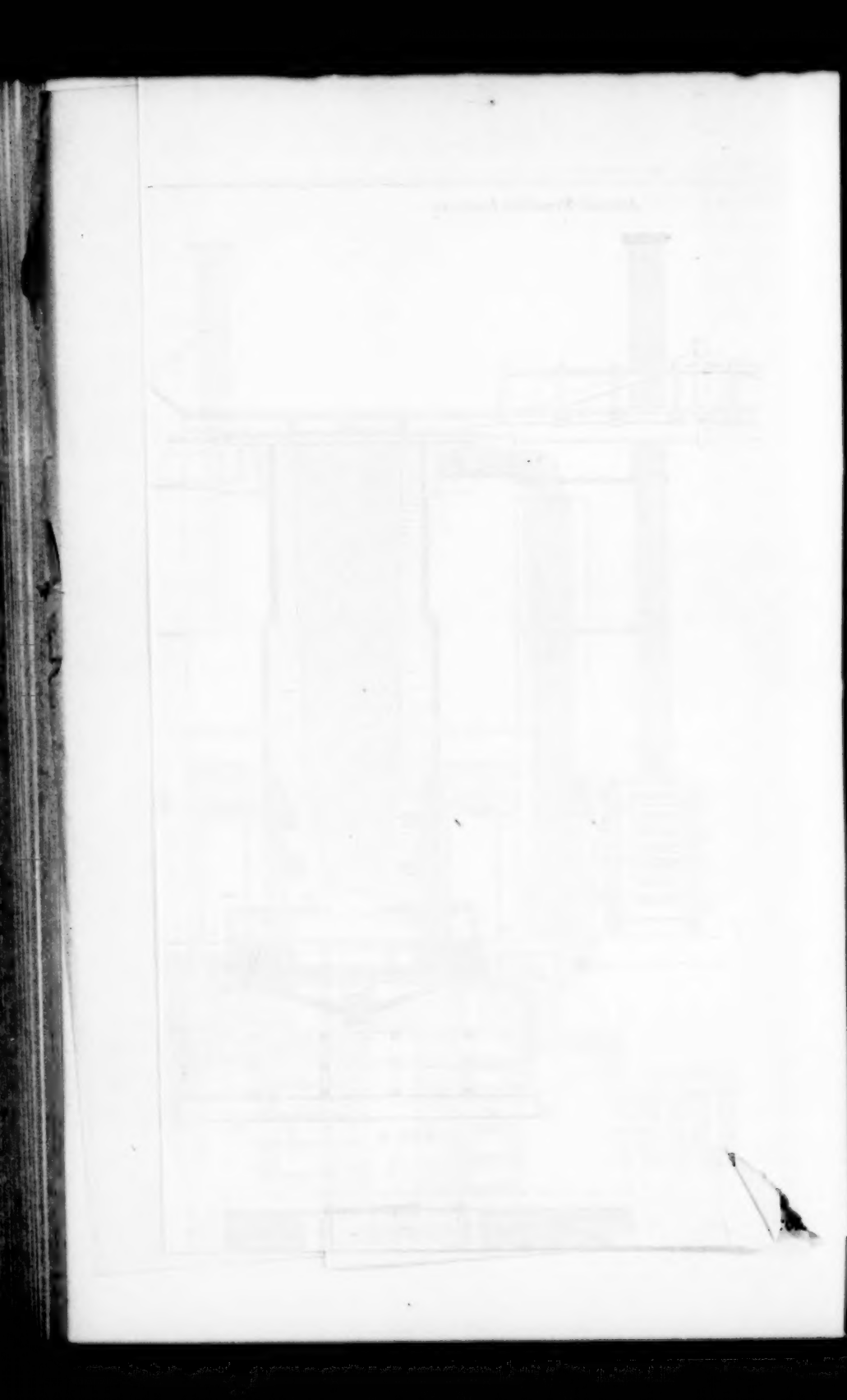


FIG. 5.



20 to 25 tons of cast iron per day, was built *in four months*, and *at once* set to work.

From these data the above stated advantage is at once apparent.

II. *A greatly increased productiveness*, amounting in some instances *to more* than *three times* the old furnaces, *and a saving in fuel and blast.*

An iron furnace of the old round form at Nischne Tagilsk of 5907 *cubic feet capacity*, 51 feet high, worked on magnetic ore containing 67·5 per cent of metallic iron with 4578 cubic feet of charcoal gave 22 *tons of gray pig iron per* 24 *hours.*

A Rachette furnace of 2125 cubic feet capacity, *only* $22\frac{2}{3}$ *feet high* with 12 tuyeres, worked with a consumption of 5341 cubic feet of charcoal the same ore to an average yield of 30 *tons* per day.

The Mulheim Rachette furnace of about 2800 cubic feet capacity yielded on the *thirteenth* day of its working *over* 21 *tons* of the best gray pig iron, and this too *with poor ore*, and with a consumption of $1\frac{1}{2}$ ton of coke to 1 ton of iron. Moreover the ores were such that from 50 to 55 tons of limestone flux had to be used per 100 tons of ore.—With respect to the saving of fuel in this furnace it must also be stated that it only requires from $1\frac{1}{4}$ to 2 *pounds pressure*, while the furnaces of the ordinary form require a blast of double and three times that pressure. Hence at the new Mulheim furnace *half the fuel* formerly required for generating steam for the blast engines *is now saved.* These results are mostly due to the peculiar structure of the furnace.

Being *low*, and possessing *an extended and well regulated* smelting zone, (an advantage that none of the several furnaces in use, round, oval or otherwise shaped can present,) further, being widened at the top, (an advantage Truran, one of our best authorities in iron matters, has fully acknowledged), *the ores only remain seven hours* in the furnace, while the old system of furnaces they remain from 16 to 24 hours.

III. *A better and more uniform quality of pig iron.*

The reported results obtained in Russia and Germany speak unanimously of the good and uniform metal obtained in the Rachette furnace, even as has recently been demonstrated, from an inferior and poor ore.

IV. *An easier management of the process, and less liability to get out of order, while at the same time a better preservation of the furnace is insured.*

The drying and heating arrangement, by means of channels and flues, extending from under the hearth through the walls upwards to the top of the furnace, has been found of great value.

Not only is the furnace thereby at a small cost dried out and heated *from the outside inside*, but it serves also, when the furnace is at work, to ventilate it, and to carry off the excessive radiated heat, and prevents the furnace from becoming overheated. This arrangement fills a third important office, namely that of making it possible to rid the furnace from the so-called scaffolding, (what however no good iron

master should allow to take place) I mean thereby the adhering of iron to the sides of the furnace. The flues are so arranged that by closing up certain apertures the heat can be concentrated on any part of the furnace and so the scaffolding melted down without recurring to the expensive blowing-out remedy.

The number and peculiar arrangement of the *tuyeres* deserve also some consideration. They are not placed opposite each other—but in an *interchanging alternate order*. A regular and continuous *smelting zone* is thereby obtained, while at the same time the peculiar rotary motion that is imparted to the smelting particles facilitates their contact with the carbon of the fuel to a much better extent than the oval or circular system. *Five to six tuyeres* have hitherto been considered best adapted for *iron* smelting about 2 feet apart.—For *copper* and *lead* 16 to 24 *tuyeres* have been employed to advantage. The inventor has also successfully used so called "*slit blast boxes*" whereby the wind is introduced in one continuous sheet. The manner of charging the furnace is the same as used in others, except that the ore and flux are fed more around the outer edges of the tunnel head, while the coal is placed more toward the middle. The flux and ore are thereby brought more into immediate contact with the blast and smelting zone, leaving the fuel more toward the middle. This has been found to give a decided improvement in the saving of fuel and more perfect reduction of the ore.

The following dimensions have been found by Mr. Aubel to be the best adapted for *iron furnaces*:—

Height about 30 feet; width of hearth at the base $2\frac{3}{4}$ to 3 feet; length of hearth 16 to 18 feet; width at the top about 7 feet.

We will now consider the success which has been obtained by the use of the Rachette furnace for the reduction of *copper* and *lead ores*.

In a *copper*-smelting furnace of this construction in Russia $53\frac{1}{2}$ *tons* of copper ore were smelted *in* 24 *hours*, while in a furnace of the old construction not more than $8\frac{1}{2}$ *tons* could be produced in the same time. A furnace of this kind therefore replaces 6 to 7 furnaces of the old system. Comparative tables, sent from St. Petersburgh show further remarkable economy in fuel (from 25 to 33 per cent.) and a great saving in labor:—

The working of the Rachette furnace has been found to be much easier and the slags more regular and about 0, 1 per cent poorer than those taken from the old furnaces. The duration of a smelting period in Russia is usually 50 to 60 days, the Rachette furnace however, has made a working of more than 140 days without interruption.—As was the case with the iron furnaces, the attention of eminent German metallurgists was also directed to the adaptation of the Rachette furnace for smelting *copper* and *lead*.

Mr. O. Hartman in Saalfield, Germany, communicates some interesting trials which he has made with a diminutive Rachette furnace for smelting of copper. This furnace has a length of $4\frac{1}{2}$ feet below and above, is 10 feet high, 3 feet wide at the hearth, and $4\frac{1}{2}$ feet wide at the top.

These tuyeres on each side supply the blast; the hearth from the fire wall to the front has a fall of 4 inches and the tuyeres are so placed as to correspond with this fall and are 23 inches above the hearth. The blast is supplied by a rotary fan-blower of 3 feet diameter, making about 1100 revolutions per minute. The result is reported to be of a most satisfactory nature. In 24 hours 15 to 20 *tons* of ore were worked up, and the fullest satisfaction in every respect obtained.

Another furnace on the Rachette system has recently been built at the Altenau, (Germany) silver works for smelting of *lead* ores and is now in most successful operation.

This furnace is 20 feet high, 7 feet 4 inches long, 3 feet below and $4\frac{3}{4}$ feet wide on the top. 5 tuyeres on each side ($1\frac{1}{2}$ in. diameter), furnish the blast about 16 inches above the hearth. It was set to work on the 28th of June this year, and has smelted up to the 2d of July, $10\frac{1}{2}$ *tons of lead* with the average consumption of 1 *pound of coke to* 10 *pounds of ore*, while another furnace of the *old* construction *did not* in the same time produce more than 4 *tons and* 150 *tons of lead* with an average consumption of 1 *ton of coke to* 7 *tons of ore.*

Mr. Aubel mentions also the adaptability of the Rachette furnace for the smelting of *cast steel*, either from the ore or from the adapted materials, such as wrought and cast iron, or cast iron and oxides. I beg to call your attention more particularly to this point of importance. The above named gentleman states that he has succeeded in melting *Platinum* before the mouth of a tuyere;—there is therefore no reason why this class of furnace will not answer for the production of large masses of steel.

I learn that the assignee in this country, Mr. Alexander Trippel, chemist at the Lehigh Zinc works at Bethlehem, Pa., has invited Mr. Charles Aubel to come to this country and assist in the introduction of the Rachette furnace. Several parties have already declared their intention to build furnaces on this new system and a smelting work is contemplated in the neighborhood of New York where furnaces for smelting lead and copper on this principle will be erected.

The great importance of this improvement to all iron masters in its economy and results has induced the owner of the patent in this country to make arrangements to form a company of such parties as will at once interest themselves in the introduction of the system.

Those wishing further information can address Mr. A. Trippel, Bethlehem, Pa., A. L. Fleury, care National Hotel, New York, or C. B. Norton, Esq., 151, Broadway, New York.

DESCRIPTION OF PLATE V.

Fig. 1 Front View.

" 2 Vertical Section. *A*, the interior; *a*, *a*, tuyeres; *b*, the chamber; *c*, *c*, flues.

" 3 Longitudinal Section.

" 4 Horizontal Section of the Hearth. *A*, the hearth; *a*, *a*, the tuyeres.

" 5 Horizontal Section of the Hearth. *c*, the heating chamber; *b*, the flues.